FEB 17 1948

The SCIENTIFIC MONTHLY

PERIODICAL ROOM
GENERAL LIBRARY
UNIV. OF MICH.

VOL. LXVI *March 1948* NO. 3

Official U. S. Navy Photograph

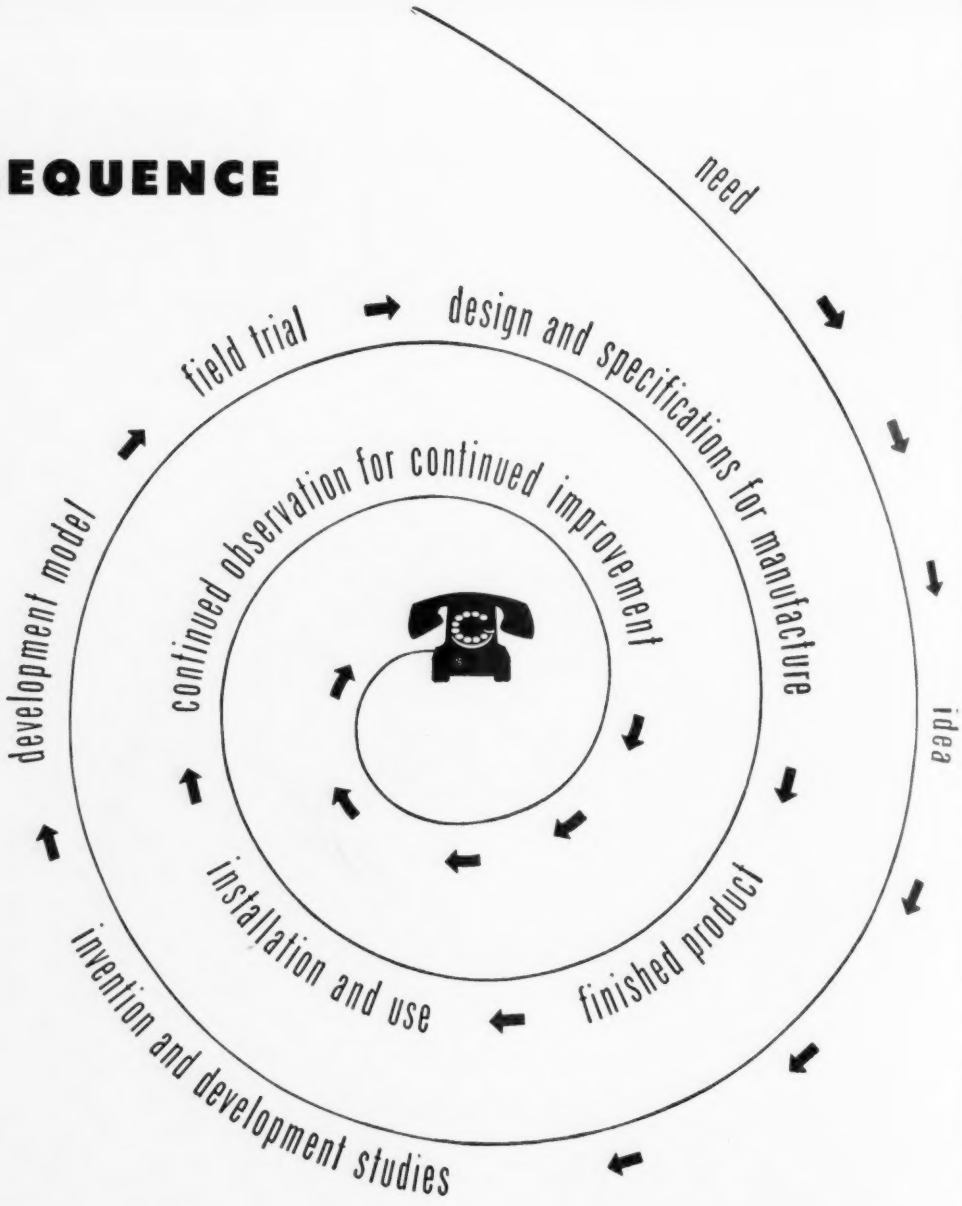
SITE NEAR MONTEREY, CALIFORNIA, FOR THE U. S. NAVAL POSTGRADUATE SCHOOL

See *The U. S. Naval Postgraduate School*, page 183.

PUBLISHED BY
THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

UNIVERSITY OF MICHIGAN LIBRARIES

SEQUENCE



At Bell Telephone Laboratories, more than 2300 scientists, engineers, and their associates are continually exploring and inventing, devising and perfecting for improvements and economies in telephone service.

BELL TELEPHONE LABORATORIES

WAR SURPLUS OPTICAL BARGAINS

ASSEMBLE YOUR OWN BINOCULARS!

Complete Optics! Complete Metal Parts! Save More Than ½ Regular Cost

GOV'TS 7 X 50 BINOCULARS

Complete Optics, new and ready for assembly. We supply full instructions. Limit—1 Set of Metal Parts and 1 Set of Optics to a customer. **METAL PARTS** — Set includes all Metal Parts—completely finished—for assembly of 7 X 50 Binoculars. No machining required. Bodies have been factory hinged and covered. A sturdy Binocular Carrying Case is optional with each set of Metal Parts.

Stock #842-X **$39.40**
Postpaid, plus $4.80 for Case—**Total** **$44.20**

OPTICS—Set includes all Lenses and Prisms you need for assembling 7 X 50 Binoculars. These are in excellent condition—perfect or near-perfect—and have new low reflection coating.

Stock #5102-X ..... **$25.00 Postpaid**

NOTICE! If you buy both Binocular Optics and Binocular Metal Parts, add 20% Federal Excise Tax to above prices.

ARMY'S 6 X 30 BINOCULARS

COMPLETE OPTICS & METAL PARTS — Model M-13A1, 6 X 30 Binoculars (Waterproof Model). Everything you need—ready for assembly. When finished will look like a regular factory job costing $102 to $120. The Optics are new, in perfect or near-perfect condition. Have new low reflection coating. Metal Parts are new and perfect, all completely finished. No machining required. Bodies factory hinged and covered. Complete assembly instructions included.

Stock #830-X ..... **$40.00 Postpaid, plus $8.00 tax—Total—$48.00**

METAL PARTS ONLY—Model M-13A1, 6 X 30 Binoculars. No Optics. Same Metal Parts as described for Stock #830-X.

Stock #832-X .. 6 X 30 Metal Parts .. **$25.00 Postpaid**

OPTICS FOR 6 X 30 BINOCULARS (No Metal Parts). Perfect, and low reflection coated.

Stock #5130-X ..... **$15.00 Postpaid**

SAME OPTICS AS ABOVE—coated—but slight seconds.

Stock #5124-X ..... **$12.75 Postpaid**

NOTICE! Add 20% Federal Excise Tax to above prices if you order *both* Binocular Optics and Metal Parts.

Optics and Metal Parts are Available for Monoculars (½ a Binocular). For Details Write for Bulletin #14-X.

6 POWER GALILEAN TELESCOPE—(Commercial Surplus) 28mm dia. Achromatic Objective Lens. Sturdily constructed of Aluminum. 7" long, extends to 9". Complete with carrying case.

Stock #941-X ..... **$3.00 Postpaid**

SIMPLE LENS KIT—Some gov't surplus, some commercial surplus, some chipped rejects, some perfect, few coated . . . in any event an excellent bargain. Containing Plano Convex, Double Convex, Concave, Miniscus Lenses. Dia. range 5mm to 40mm. Assorted focal lengths.

Stock #5-X ..... 45 lenses ..... **$5.00 Postpaid**
Stock #10-X ..... 80 lenses ..... **$10.00 Postpaid**

$200.00 DRIFT METER for $4.95

These were used for determination of drift and true air speed. You can adapt to other uses or take apart to get 2 mounted Achromatic Lenses—Mirrors—Field Lens—Pantograph—Engraved Scales — Sponge Rubber Discs — metal parts and other components. Instrument weighs 4 lbs.

Stock #936-X ..... **$4.95 f.o.b. Chicago, Ill.** (Shipping weight 12½ lbs.)

WE HAVE LITERALLY MILLIONS OF WAR SURPLUS LENSES AND PRISMS FOR SALE AT BARGAIN PRICES WRITE FOR CATALOG "X"—SENT FREE!

NEVER HAS THERE BEEN SUCH A SENSATIONAL BARGAIN AS THIS!

BUBBLE SEXTANT

These Army Air Forces Bubble Sextants cost the Govt. about $125.00 each. A real bargain at our price of $12.50! Included with Bubble Sextant shipment is wooden Carrying Case, 5 spare waxed paper discs, flashlight with rheostat for night use (uses ordinary flashlight cells — not furnished) aux. 2-power Galilean Telescope, Allen wrench, 1 spare marking point. Wartime used, **BUT COMPLETELY REPAIRED, COLLIMATED AND PUT IN GOOD WORKING ORDER.** If not satisfied that Sextant is exactly as represented, return within 10 days and money will be refunded. Full directions for use accompany each shipment.

Stock #924-X ..... **$12.50 Postpaid**

Same Sextant as above, but **BRAND NEW** and with Automatic Electric Averaging Device and Illuminated Averaging Disc for nighttime use. Govt. cost $217. Though brand new we have rechecked Bubble and Collimation and GUARANTEE perfect working order.

Stock #933-X ..... **$22.50 Postpaid**

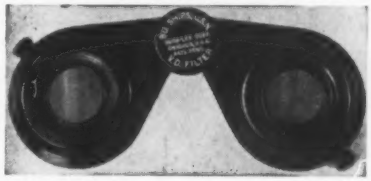
AIR FORCES GUN SIGHT—With Polarizing Variable Density Attachment. (Polarizing attachment alone is worth many times the price of entire unit.)

Stock #908-X ..... **$5.00 Postpaid**

Same Unit Without Polarizing Attachment

Stock #916-X ..... **$2.50 Postpaid**

POLARIZING VARIABLE DENSITY ATTACHMENT FOR BINOCULARS



An amazingly effective unit for controlling amount of light reaching your eyes. Cuts down glare in sky and overwater observations. Easily snapped on and off over the eye cups of American made 7 X 50 Binoculars. Govt. cost, $8.30 each.

Stock #20,000-X ..... **$2.00 Postpaid**

Above Polarizing Attachment also adaptable to Govt. 6 X 30 Binoculars with substitution of special Eyecups.

Stock #20,010-X .. 6 X 30 Eye-cups .. **75¢ pr. Postpaid**

CONDENSING LENSES—Seconds, but suitable for Enlargers and Spotlights.

Stock #1061-X .. 6½" dia., 9" F.L. .. **$2.50 Postpaid**
Stock #1082-X .. 4½" dia., 5½" F.L. ... **70¢ Postpaid**

We Have a Limited Quantity of Condensing Lenses—seconds—ranging from 4½" to 8½" in dia. with various focal lengths. Send for Free Bulletin titled **"MISCELLANEOUS CONDENSING LENSES–X".**

MOUNTED PROJECTING LENS SYSTEM—F.L. 91.44 mm. (just right for 35 mm. Projectors). Speed of F. 1.9. Outside dia. of mount at one end 60 mm. Length of mount 64 mm.

Stock #4033-X ..... **$3.00 Postpaid**

THREADED OUTSIDE MOUNT FOR FOCUSING ABOVE SYSTEM (Not Illus.)—

Stock #715-X ..... **$1.00 Postpaid**

CONDENSING LENS SET—Consists of 2 Condensing Lenses with diameter of 2". Used for making 35mm projectors or many other purposes.

Stock #4034-X ..... **75¢ Postpaid**

Satisfaction Guaranteed

EDMUND SALVAGE COMPANY, P. O. Audubon, N. J.

THE SCIENTIFIC MONTHLY

VOL. LXVI MARCH 1948 NO. 3

F. L. Campbell, *Editor* Gladys M. Keener, *Assistant Editor*
Theo. J. Christensen, *Advertising Manager*

Contents

The U. S. Naval Postgraduate School	*Ford L. Wilkinson, Jr.*	183
George Brown Goode (1851–1896)	*Paul H. Oehser*	195
Time	*George G. Ensign*	206
Teamwork in Industry	*E. Wight Bakke*	213
Eighty-eight Years of Synthetic Rubber	*Ralph F. Wolf*	221
A Parable	*K. Starr Chester*	231
Principles of Really Sound Thinking	*Max Black*	232
Arctic Tern (Verse)	*Mae Winkler Goodman*	234
Man's Control over Civilization	*Leslie A. White*	235
Well of Desolation (Verse)	*Katharine O'Brien*	247
Cultural Discontinuity	*Jules Henry*	248
Science on the March: Centrifugal Fields	*J. W. Beams*	255
Book Reviews		259
Comments and Criticisms		267
Technological Notes		271
The Brownstone Tower		272

Centennial Celebration, A.A.A.S., Washington, September 13–17, 1948

The American Association for the Advancement of Science assumes no responsibility for the statements and opinions advanced by contributors to its publications. Views expressed in the editorials are those of the editor and do not necessarily represent the official position of the American Association for the Advancement of Science.

Published monthly at James F. McCarthy, Successor to The Science Press Printing Co., by the American Association for the Advancement of Science under the direction of the Publications Committee: Roger Adams, J. E. Flynn, Kirtley F. Mather, Walter R. Miles, W. J. Robbins, and Malcolm H. Soule. Office of publication, North Queen St. and McGovern Ave., Lancaster, Pa.

Orders for subscriptions and requests for change of address should be directed to the Circulation Department, A.A.A.S., N. Queen St. and McGovern Ave., Lancaster, Pa., or 1515 Massachusetts Ave., N.W., Washington 5, D. C. Four weeks are required to effect changes of address. Address all correspondence concerning editorial matters and advertising to the Office of The Scientific Monthly, 1515 Massachusetts Ave., N.W., Washington 5, D. C. Subscriptions: $7.50 per year; single numbers, 75 cents. Copyright 1948, by the American Association for the Advancement of Science. Entered as second-class matter at the Post Office at Lancaster, Pa., December 30, 1947, under the Act of March 3, 1879. Acceptance for mailing at the special rate of postage provided for in the Act of February 28, 1925, embodied in paragraph 4, Sec. 538, P. L. and R., authorized December 30, 1947.

THE
SCIENTIFIC MONTHLY

MARCH 1948

THE U. S. NAVAL POSTGRADUATE SCHOOL

FORD L. WILKINSON, JR.

Dr. Wilkinson graduated from the U. S. Naval Academy in the class of 1918 and served in the Navy until 1927. In March 1947 he went to the U. S. Naval Postgraduate school as its first civilian dean. He had previously held the position of dean of the Speed Scientific School at the University of Louisville.

RECENT legislation by Congress has provided the Navy with an institution that may well become one of the nation's outstanding graduate schools in science and technology. The two acts passed by the Eightieth Congress, and enacted into laws by the signature of the President, provide the U. S. Naval Postgraduate School with statutory authority to conduct graduate and postgraduate instruction in an unlimited variety of fields and to establish the School as an independent unit with its own superintendent and with administrative authority similar to that of the U. S. Naval Academy; they authorize also the purchase of property at Monterey, California, for the future home of the School.

Although the legislation referred to marks an important milestone in the history of naval officer advanced education, its major effect is to recognize by legislation a program that has been under way under executive order since 1909 and to give to that program a permanent place in the education of officers of the Navy, the Marine Corps, and the Coast Guard. In addition, it provides for the purchase of a property of adequate size in a location ideally suited for the conduct of a year-round program of intensive study. Although the legislation authorizes only the purchase of the property and makes it possible to convert the buildings already on the site to immediate usefulness by a limited activity of the School, future plans call for a transfer of all the School's activities to Monterey as soon as funds are available and a suitable physical plant can be constructed. It is believed that the procurement of the Monterey site is the first time in the history of education that an institution of higher learning has been established in a locality selected solely on the basis of environmental conditions.

History of the Postgraduate School. The Naval Postgraduate School was organized in 1909 as a division of the U. S. Naval Academy, with an officer in charge who was designated as head of the School. It has operated under this plan with its own instruction staff since that date, except for two years during World War I. It has occupied its own quarters on the Naval Academy grounds since 1919, and its physical plant has until recently been adequate for the number of student officers enrolled.

Official U. S. Navy Photograph

DR. FORD L. WILKINSON, JR.

ACADEMIC DEAN, U. S. NAVAL POSTGRADUATE SCHOOL.

The separation of the School from the Naval Academy has been necessitated by the tremendous increase in demand for scientific and technical graduate education of naval officers and because of the inability of the School to expand its present physical plant. Hereafter, graduate and advanced education will be a function separate from undergraduate education.

The School consists of three major divisions: the Engineering and Scientific Division, now at Annapolis; the General Line School at Newport, Rhode Island; and the Naval Intelligence School at Anacostia, D. C. Each of the three units is relatively independent at present, but under the general administrative direction of the superintendent of the U. S. Naval Postgraduate School. It is expected that all three of the major divisions will be combined in one location when facilities are made available at Monterey.

In addition to the three separate schools for which the Navy supplies complete faculties and physical facilities, the superintendent of the U. S. Naval Postgraduate School directs the educational activities of naval officers conducted entirely at civilian institutions. These programs, usually involving small groups of officers, are at present in the fields of naval construction, civil engineering, law, theology, textile engineering, business administration, personnel administration and training, and industrial accounting. The programs extend over periods of a few months for certain special subjects to three years for naval construction. Each is given under the specific curriculum of the institution to which officers are assigned and is conducted under the educational supervision of the faculty of that institution. The program in each of the participating colleges and universities is reviewed by the administration of the Postgraduate School in conjunction with the naval activity initiating the program and the Division of Training of the Bureau of Naval Personnel.

Inasmuch as the largest concentration of graduate and professional studies is within the three major divisions operated under their faculties, the organization and activities of these divisions will constitute this story of the U. S. Naval Postgraduate School.

General Line School. For some years prior to World War II, junior officers were ordered to the Naval Postgraduate School at Annapolis for a one-year course in purely professional subjects. It was the intent of the Navy Department to utilize this period in the conduct of an intensive program of professional training in all activities of the line officer, with limited applications in the technical fields of marine engineering and communications. It was undertaken to offer to each young officer on his first tour of shore duty an opportunity to enhance his educational background and confirm his experiences as a junior officer at sea.

An effort was made to ensure that each junior officer received one year of General Line School training within about five years after receiving his commission. Since the input of commissioned officers consisted almost exclusively of graduates of the Naval Academy, and the classes were in those days small, this work could be carried on without undue strain on the physical facilities of the Annapolis School. The staff was composed largely of naval officers teaching purely professional subjects, with members of the civilian faculty of the engineering division assisting in refresher courses in mathematics,

electrical engineering, and fundamental sciences.

The General Line School was discontinued just prior to the war because of the need for officers to man the rapidly growing fleet and because of the increased need for technically educated officers from the technical school. The physical facilities could not accommodate both programs.

The School was revived in 1946 in temporary quarters at Newport, with a new purpose and objective. The Navy Department had agreed to provide each Naval Reserve officer who transferred to the Regular Navy a year of professional training in the General Line School. Its revival in 1946 was for the purpose of carrying out this agreement. The present enrollment of approximately 600 student officers consists mainly of those officers who have been performing duties at sea of a highly specialized nature but who have had little or no training in the general-line duties of a naval officer.

It is expected that a second school for 500 additional officers will soon be established in Monterey. It is anticipated that by 1954 the tremendous backlog of transferred officers will have gone through these schools. When that is accomplished, the Newport school may be discontinued, and the General Line School at Monterey will be integrated with the U. S. Naval Postgraduate School on the prewar basis, serving graduates of the Naval Academy and those of the fifty-two NROTC units who transfer to the regular service. Its eventual capacity at Monterey will be approximately 1,250 junior officers annually pursuing a one-year curriculum.

Naval Intelligence School. Experience during the war definitely determined the need of a training school for naval intelligence, and the present school was organized and its first class started in July 1946 at the Naval Barracks at Anacostia. There are two major divisions of the Intelligence School: one is concerned with professional and strategic intelligence training, which consists of approximately nine months at the school and in the field; this is followed by concentrated foreign-language instruction extending over periods varying from three to twenty-one months, depending upon the language being studied.

Official U. S. Navy Photograph

CAPTAIN HERMAN A. SPANAGEL, USN

SUPERINTENDENT, U. S. NAVAL POSTGRADUATE SCHOOL.

The language division of the school follows the methods employed by the Navy's wartime Japanese Language School. Because of the unique methods of instruction and the high degree of concentration on the languages taught, the School has been successful in graduating a majority of its student officers with a proficiency in the language studied equal to that of a native who has an educational background at the equivalent level of a second-year college student. The success of the language division of the Intelligence School is attested by the number of governmental agencies utilizing its facilities and faculty for language training for foreign service.

As soon as is feasible, the Naval Intelligence School will also move to the Monterey location.

The Technical Division. Of major interest to readers of THE SCIENTIFIC MONTHLY is the program of the graduate division in science and engineering. The work of this division has been the backbone in the development of engineering and of scientifically trained officers since the School was first established in 1909.

UNIVERSITY OF MICHIGAN LIBRARIES

Because of the necessity of having all engineering and scientific laboratories well established and equipped before moving to a new location, it is expected that the Technical Division will remain in its present location at Annapolis for several years. It, however, will follow the General Line School and the Naval Intelligence School as early as is convenient and thus complete the establishment of the U. S. Naval Postgraduate School in Monterey.

From the very beginning of naval postgraduate education, the basic reason for operation of the Postgraduate School has been the need for a relatively large number of naval officers who possess scientific and engineering education beyond that encountered in their undergraduate work. Officers so trained have for years been directing the operation and maintenance of machinery aboard ships, directing the design and procurement divisions of the material bureaus of the Navy, initiating new technical developments, and serving as the directors of Navy tests and research. The areas and fields of study are determined by the technical bureaus of the Navy, and representatives of those bureaus assist in the development of the curriculums offered by the Postgraduate School.

Until relatively recent years, the Postgraduate School at Annapolis drew its students from the ranks of Naval Academy graduates who, after a number of years at sea, were selected for advanced educational programs under the sponsorship of the Bureau of Ships, Bureau of Ordnance, and Bureau of Aeronautics. The courses consisted mainly of a one-year curriculum designed for the purpose of bridging the gap between the professional program of study at the Naval Academy and the requirements for entrance to a graduate school of engineering at selected civilian institutions. The work at the civilian institutions was usually of a broad nature in a general field of engineering, such as aeronautical, electrical, or mechanical. It was customary on the part of most civilian institutions to award the Master's degree after one year of study in the specified subjects.

As the technical complexity of naval operation increased and the needs for officers with more specialized and scientific training became imperative, the program became more flexible and expanded in scope. Today it offers, either wholly at the School or in cooperation with civilian colleges and universities, approximately twenty-two scientific and engineering curriculums leading to the Master's degree, granted by either the Naval Postgraduate School or one of the nine cooperating institutions.

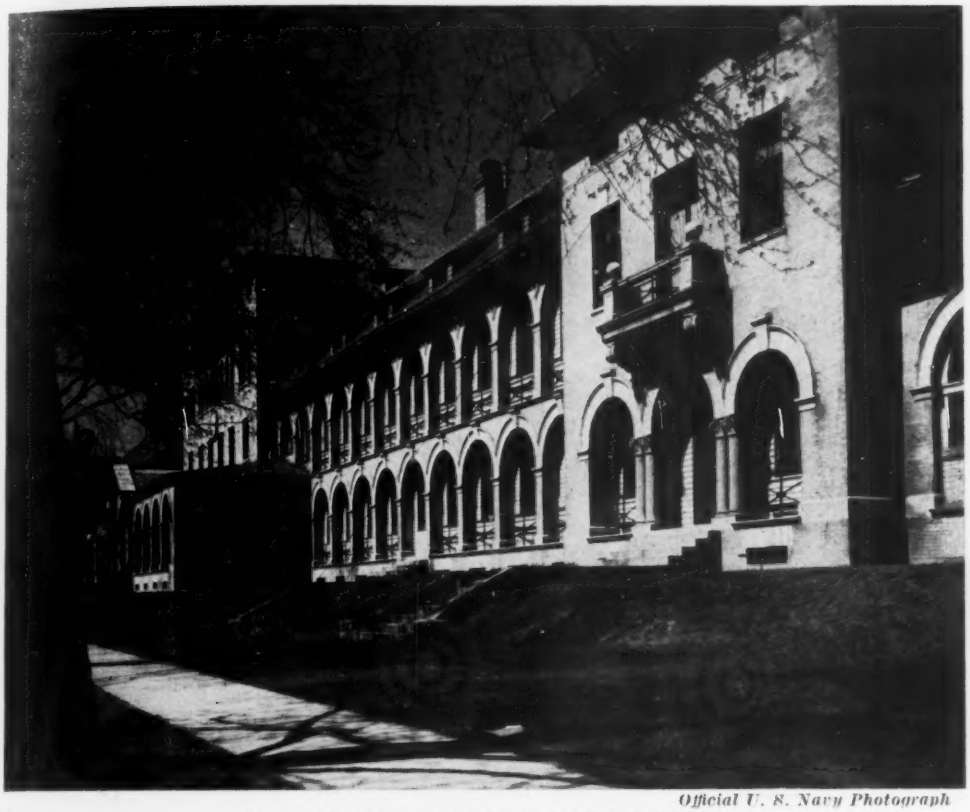
The criterion used to decide whether an entire graduate curriculum shall be offered at the School or at a selected civilian institution is based on an exhaustive study of the requirements of the course of study, and the decision is reached on the answer to the question, Where can the curriculum be taught with the greatest effectiveness?

Most of the graduate engineering and scientific curriculums are conducted over a period of three calendar years. Certain of these, such as naval construction and civil engineering, are conducted wholly at the cooperating civilian institutions. Others consist of preliminary study of one year at the Postgraduate School followed by two additional years at the School or elsewhere.

The curriculum of the first year is devoted to refresher work in mathematics, physics, and chemistry, with additional work of an advanced undergraduate nature designed to supply the deficiencies of the students' undergraduate technical and scientific studies. Because of the maturity of the students and the high degree of selection, it is possible to carry this program in the third and fourth terms of the first year to a somewhat higher level than is common in undergraduate study.

The second and third years deal rather specifically with the field of specialization undertaken by the student officers. In some fields of study these years are spent at the cooperating institutions, in others the second year is continued at the School with only the third year away, and with still others the entire three-year program is conducted at the School.

The need for specialized study is determined mainly by those divisions of the Navy dealing with design, construction, and operation of machinery, and with research and de-



Official U. S. Navy Photograph

U. S. NAVAL POSTGRADUATE SCHOOL

THIS IS THE PRESENT BUILDING ON THE GROUNDS OF THE NAVAL ACADEMY, ANNAPOLIS, MARYLAND.

velopment. Assignment to study is based on the projected needs of these divisions for specialized education beyond the undergraduate level. In addition, there are a few programs of an applied nature seeking to develop trained officers in a short period for certain operational billets at sea and ashore. These latter are called "applied curricula," are definitely at the training level, and are not recognized as involving graduate study. At present these programs, covering periods of from one to two years, are in the fields of applied communications, applied naval engineering, and applied aerology.

The distribution of student officers for the year 1946–47 under the direct supervision of the staff of the Postgraduate School was approximately as follows: 543 enrolled in curriculums conducted in whole or in part at the Annapolis School, of which 328 were taking first-year work at Annapolis, 137 taking advanced work at civilian institutions, and 78 taking advanced work at the School. The number taking advanced work at Annapolis in 1947–48 has increased to approximately 190 enrollees, with a total enrollment of all students in the engineering and science courses, either at Annapolis or elsewhere, of 570. The 570 student officers are enrolled in twenty-eight scientific and engineering curriculums, of which all but three are graduate in character and lead to at least a Master's degree granted by the Postgraduate School itself or by a cooperating civilian institution.

At present, the Postgraduate School conducts complete curriculums at the School leading to the Master's degree in engineering electronics, aerology, and naval engineering.

The staff of the School at Annapolis is composed of both naval officers and civilians,

the officers conducting courses in purely naval professional subjects, generally in the applied curriculums, and the civilian faculty in science and nonnavel technical subjects. At present there are approximately 30 naval officers and 59 civilian faculty members attached to the staff.

The division of work between the naval school and the cooperating institution is not fixed, but is determined on the basis of best utilization of the nation's graduate educational facilities. It is believed that the programs conducted wholly at the School can best be handled there because of the relatively large numbers involved and the particular marine emphasis required in the graduate program. In other fields of a high degree of specialization, involving a relatively small number of student officers, it is believed that the objectives of graduate education can best be attained by drawing on the facilities of those schools whose faculties have demonstrated outstanding competence in the field of study to be pursued. In many cases two or more institutions are selected for similar programs. The results of graduate instruction are continuously reviewed by the Postgraduate School staff, and shifts in cooperating institutions are made from time to time in order that the most effective programs may be obtained.

In the spring of 1947, the Navy Department requested the American Council on Education to appoint a Committee to "Report on the Educational Program of the United States Naval Postgraduate School." That Committee consisted of Henry T. Heald, president of Illinois Institute of Technology, chairman; Edward L. Moreland, executive vice-president of Massachusetts Institute of Technology; Baldwin M. Woods, director of University Extension and profes-

Official U. S. Navy Photograph

A SECTION OF THE ELECTRICAL ENGINEERING LABORATORY

Official U. S. Navy Photograph

METEOROLOGICAL MAPPING IN THE AEROLOGY DEPARTMENT

sor of mechanical engineering at the University of California; with Herbert A. Simon, professor of political science at Illinois Institute of Technology, as secretary.

The report of the Committee has this to say about the program of graduate study:

> We believe that the Navy has been wise in not attempting to develop completely self-contained educational facilities to meet all these training needs, but in drawing, instead, upon the resources of established civilian institutions for a substantial part of the program. . . . No general criteria appear to have been laid down to determine where the work of the Postgraduate School should leave off, and where the work of cooperating institutions should begin. However, the committee realizes that the most effective division must be determined separately for each curriculum in view of the needs of the program and the facilities both of the School and civilian institutions. Probably no single pattern will be suitable for the program as a whole.

As a general policy, the Postgraduate School has attempted to develop strong faculties in a few fields for which there is a constant and stable demand for graduate and advanced undergraduate education. These are largely in the scientific fundamentals of mathematics, mechanics, physics, and chemistry, and in the specialized fields of electrical engineering, electronics, metallurgy, mechanical engineering, meteorology, and aeronautics. Its faculty is selected on the basis of the strength of each member's training in fundamental science and mathematics, with emphasis on proved ability in creative endeavors. Members of the faculty are given privileges for research and consulting practice similar to those of an enlightened civilian institution, and they are employed under the tenure rules prevailing in American educational institutions. They are not classified civil service employees but are in an excepted class of government service.

The principal officials of the U. S. Naval

Official U. S. Navy Photograph

MICROSCOPIC EXAMINATION OF METALS IN THE METALLOGRAPHY LABORATORY

Postgraduate School at Annapolis are the superintendent, who by law must be a naval officer of rank not below that of captain; an executive officer; and the academic dean, who is a civilian.

All officer and civil service personnel are under the control and supervision of the executive officer, who is responsible for the execution of the plans and policies of the superintendent. The academic dean exercises control and supervision over all civilian faculty members and is responsible for all instruction activities except in strictly naval professional subjects.

As stated elsewhere, the staff of the U. S. Naval Postgraduate School includes both naval officers and civilians. Some of the naval officers instruct in naval subjects, and in addition there is an officer in charge of each of the curriculums of instruction. In a sense, the officer in charge of a curriculum is a liaison officer between the School and each material bureau of the Navy, but under the direct authority of the superintendent. It is his specific duty to see that the curriculum or curriculums under his supervision are so organized and conducted as to meet the educational needs of the sponsoring material bureau. Student officers assigned to a curriculum of study are under the direct supervision of the officer in charge.

The civilian faculty members are grouped into service departments of mathematics and mechanics, chemistry and metallurgy, physics and electronics, aerology, aeronautics, electrical engineering, and mechanical engineering, with a chairman, designated by the superintendent upon the recommendation of the academic dean. A civilian faculty member, who may or may not be a department chairman, is designated as an academic associate with each curriculum officer. He has

joint responsibility with the officer in charge of curriculum for the formulation of curriculum course content and sequence at the School and at the cooperating institution.

Curriculum building is in reality a joint endeavor of naval officers, civilian faculty, and cooperating institutions. Each curriculum is reviewed by the academic dean and approved by the superintendent. The strong participation of the civilian faculty in the design of curriculums and the conduct of courses provides the needed continuity that might be lost were these functions left wholly to members of the naval officer staff, whose tour of duty cannot extend beyond a limited number of years.

The civilian faculty is an organized body under the chairmanship of the academic dean and functions as an advisory body on academic matters. An Academic Council, consisting of the academic dean, chairmen of service departments, and the senior officer in charge of curriculum, has complete authority over policies and standards with regard to the granting of degrees.

The following excerpts from the American Council on Education report relating to academic policies and faculty organization will be of interest:

> We must emphasize again that the purpose of the School is to meet the training requirements of the Navy. There is no need for the School to be concerned with civilian academic standards unless the maintenance of such standards will contribute to the training objectives of the institution. For several reasons, however, this Committee believes that the School can accomplish its objectives in the field of scientific and engineering education only by adopting the general educational outlook and approach of a good graduate school.
>
> First of all, while very few of the men who pursue these curricula will ever be engaged in the actual conduct of research, many of them, in their naval career, will be responsible for the administrative direction of research organizations, or for

Official U. S. Navy Photograph

PART OF THE INTERNAL-COMBUSTION ENGINE LABORATORY

making basic decisions with respect to the research and development policies of the Navy. They will be in strategic positions of liaison between scientific activity and its application to naval problems. On them will depend heavily the Navy's ability to make use of developments in American science and technology. In addition to their naval professional qualifications, these men will need to be possessed of three abilities: administrative capacity, sufficient scientific and technological training to understand the language of the scientist and to appreciate the applications of his work, and a thorough appreciation of the nature of scientific research, of the attitudes of mind and methods of work of the research man, and of the conditions under which research and development work can most fruitfully be carried on. Academic work, as it is conducted in graduate schools is the most effective method—perhaps the only method—of developing the second and third of these abilities.

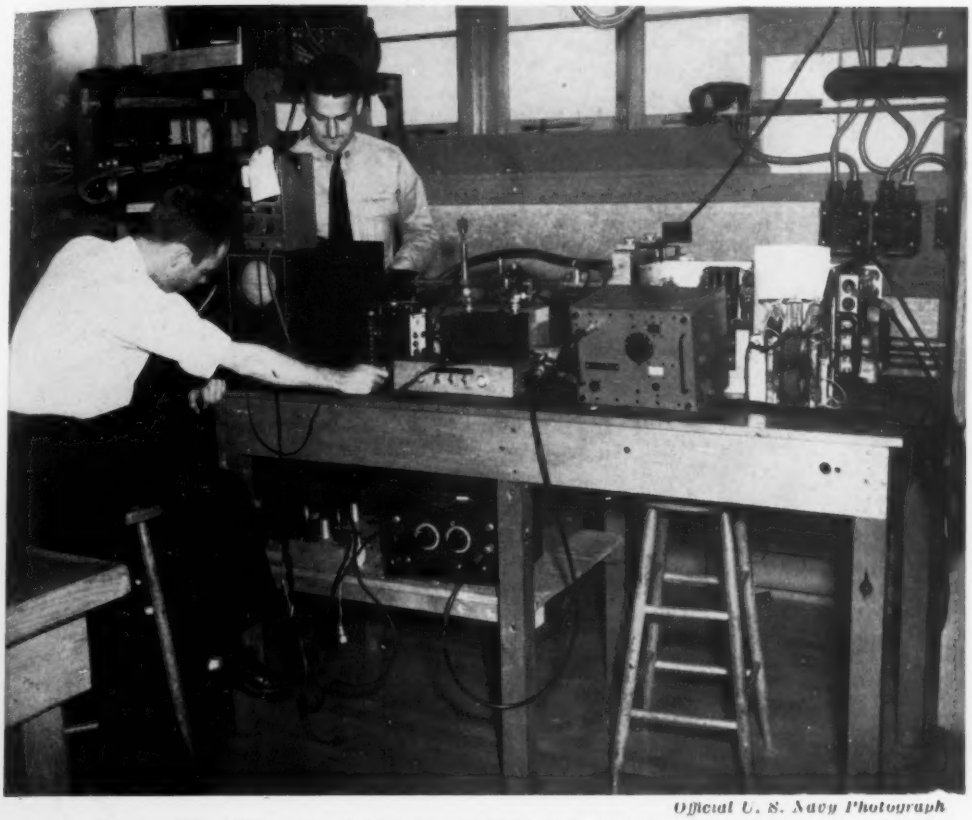
Secondly, the only way in which the Postgraduate School can secure and retain a Faculty competent to conduct scientific and engineering instruction at an advanced level is by providing the kind of working environment that attracts such men to an academic career.

[Concerning the organization of the School, the Committee further states]: The Committee, therefore, believes that the . . . scheme of organization is entirely appropriate, and that the civilian faculty members will have as much freedom and as broad authority and influence over academic policy as can be expected in a school whose primary function is the training of officers for professional duty.

THE present organization and faculty policies have been established by Captain Herman A. Spanagel, USN, formerly head of the Naval Postgraduate School, and at present superintendent of the newly authorized U. S. Naval Postgraduate School. He early recognized the demands that will be made on the School during the postwar period and forecast with unusual foresight the character of school necessary to bring this about. It was fortunate indeed that the Navy Department and its technical divisions had the strong support of as persevering a person as

Official U. S. Navy Photograph

A CORNER OF THE RADAR LABORATORY



Official U. S. Navy Photograph

STUDENT OFFICERS ENGAGED IN HIGH-FREQUENCY RESEARCH PROJECT

Captain Spanagel in launching its expanded program.

An analysis of the postwar continuing needs for graduate training by the material bureaus indicated in early 1945, even before the end of hostilities, that the physical facilities of the existing School could not possibly fulfill the requirements. Captain Spanagel set machinery to work in planning for the future. In this he had the wholehearted support of the Secretary of the Navy and virtually every chief of bureau in the Navy Department. Where legislation was needed to implement plans, the Navy Department sought it of Congress. It is interesting to note that the entire program of the Navy Department for postgraduate education, as proposed by a Board of which Captain Spanagel was Chairman, has been enacted into law by either the present or preceding Congresses.

It is recognized that several years will ensue before the full fruition of the plans laid down. For instance, the present plant facilities at Annapolis will accommodate an uncomfortable maximum of 500 student officers. The ultimate plans, based on the established needs for graduate education in science and technology, call for an annual enrollment, not including those at cooperating civilian institutions, of approximately 800 student officers.

It is planned that the physical plant to be constructed at Monterey will accommodate about 2,900 student officers, distributed as follows: 1,250 in the General Line School, 300 in the Naval Intelligence School, 800 in the graduate scientific and engineering curriculums, and 550 in several forms of applied engineering.

Laboratories will be established in all the fundamental sciences and in the several fields

of technology in which both graduate and undergraduate instruction will take place. A staff of several hundred naval officers and civilian instructors will be required.

In order that the faculty may engage in creative work, the Postgraduate School at present offers a rapidly strengthening program of fundamental research and development work for its staff and student officers. The facilities for this work will be greatly expanded in its future home, and it is confidently expected that a reasonable portion of the Navy's vast research program will be allotted to the staff of the Postgraduate School.

Theses are required of all degree candidates either at cooperating institutions or at the U. S. Naval Postgraduate School. These are of an experimental or theoretical nature and are undertaken in an attempt to solve some of the research and development problems of sponsoring technical bureaus or as supplemental solutions to parts of research projects assigned to the School by activities of the Navy Department. Allocation of funds for long-range research programs will be made on a contract or project order basis with the School in much the same manner as the technical bureaus and the Office of Naval Research contract for the services of civilian colleges and universities.

It is anticipated that future appropriations for operation of the U. S. Naval Postgraduate School will carry adequate funds for the conduct of independent research, thus ensuring continuing support of this important function of a graduate school.

The standards for awarding the Master's degree are somewhat stricter than those in a civilian institution. They were purposely made strict in order that there may be no question as to the character of the degree awarded. Authority has been granted by Congress to award the doctorate degree to worthy candidates. While this degree has never been awarded by the School, and if ever awarded will undoubtedly be restricted to only a few special students, much of the work available may be considered of a character that would be sufficiently advanced for doctoral candidates.

The American Council on Education Committee, while advising against the extension of the program beyond the Master's requirements, has this to say about the graduate program conducted at the School: "The Committee finds the regulations governing the award of the Master's degree generally acceptable." Its recommendations concerning the award in general require no adjustment of the standards previously established. In fact, the present requirements might be lowered in certain respects and meet with the recommendations made.

On the other hand, the Committee found in its study that the physical facilities of the present school materially hampered the School in the implementation of its scientific and technical program, and it made many constructive suggestions that, when carried out, will unquestionably give the U. S. Naval Postgraduate School an unusually strong opportunity to lead in the field of advanced and graduate instruction.

It is not anticipated that the growth of the physical plant and the expansion of the faculty of the School will curtail the utilization of cooperating civilian institutions in the Navy's program of graduate training. The contemplated expansion will result, however, in an increase in the number of students and of graduate programs, and in greater versatility in meeting the increasingly diverse needs of the Navy.

GEORGE BROWN GOODE (1851–1896)*

PAUL H. OEHSER

Assistant chief, Editorial Division, Smithsonian Institution.

POSSIBLY no other man connected with the Smithsonian Institution has ever possessed such a marked ability to pick sound and capable assistants as Spencer Fullerton Baird. He was quick to perceive the latent worth of a man. Just as at Cambridge Louis Agassiz, by virtue of his magnetic personality and unique teaching methods, attracted a whole school of American students of natural history, so Baird at Washington gathered around him a coterie of young and promising talent. Baird, of course, was their master, and together they became known as "the Bairdian school," characterized by exactitude in scientific statement, conciseness in deduction, and careful analysis of data, in contrast to many of the older European naturalists whose statements often had to be taken without benefit of documentary substantiation.

There was one man who came to the Smithsonian under Baird's aegis who turned out to be a genius, especially in his broad and humane interests and in the quality and quantity of scientific and literary work that he performed in his brief forty-five years of life. If George Brown Goode had lived, quite likely he would have become secretary of the Smithsonian. David Starr Jordan said: "Of all American naturalists, Doctor Goode was the most methodical, the most conscientious, and, in his way, the most artistic. And of them all no one was more beloved by his fellows." It was, as Secretary S. P. Langley remarked, "incomprehensible that one man could accomplish so much in one single life."

Goode was born at New Albany, Indiana, on February 13, 1851, of sturdy American stock. His mother, Sarah Woodruff Crane, died in the boy's infancy. He passed his early youth at Amenia, in eastern New York, where his father had moved after retiring from his mercantile business. We are told that Francis Collier Goode, the father, was of studious habits and keen observation and that he bore a strong facial resemblance to Henry Clay. Though he lacked a professional education, he was well informed. A friend said of him: "Few public men are better acquainted with the political history of the United States, and few naturalists more enthusiastic lovers of nature, or more familiar with the habit and appearance of plants." These characteristics he passed on to his son. It is of interest, too, that in his library he had a set of "Smithsonian Reports," and from these young George first became attracted to science and learned of the Institution he was later to serve. It seemed to be an instance of boyhood dreams come true.

In 1870, when only a little past nineteen, Goode was graduated from Wesleyan University at Middletown, Connecticut, with a decided penchant and talent for natural history. The same year he began postgraduate work at Harvard College under Louis Agassiz and so distinguished himself with the professor that he was recommended for the position of curator of the university museum in the Judd Hall of Natural Science at Middletown. This building had just been presented to Wesleyan University by Orange Judd, the pioneer agricultural journalist and Goode's future father-in-law. Goode undertook the arrangement of the natural-science collections and gained therefrom an experience in museum administration and techniques that was to prove a solid foundation in later years. He was then only about twenty years old, a stout and healthy lad, prodigiously doing the work of an older man and doing it well.

* This article will form part of a chapter in the author's book, a biographical history of the Smithsonian Institution, to be published by Henry Schuman, Inc., New York, in "The Life of Science Library." Photographs, courtesy of the Smithsonian Institution.

His first appearances in print consisted of various short notes in the *College Argus* and *College Review,* but his maiden scientific paper, "The Billfish in Fresh Water" (i.e., the Connecticut River), appeared as a brief note in the *American Naturalist* in September 1871. At the Portland meeting of the American Association for the Advancement of Science in 1873 he presented a paper on the question "Do Snakes Swallow Their Young?" and this, Goode's first important contribution to zoology, was published the next year in the *Proceedings* of the Association.

In 1872, through the influence of his stepmother, he met Professor Baird at Eastport, Maine—a meeting that proved to be a turning point in his life. The following year, at the Portland meeting of the A.A.A.S., the two met again, and Baird, at once recognizing Goode's genuine potentialities, invited him to participate in the work of the United States Fish Commission, which was then beginning to take shape under Baird's organizing genius. The result was that the young man began a volunteer service for the Commission that lasted several years, during which he collected fishes along the eastern coast, wrote papers on what he was learning about them, and became not only a specialist in technical ichthyology but also a student of the economic problems relating to fisheries and the fishing industry. Upon Baird's invitation he spent the winter of 1873 in Washington arranging the fish specimens at the Smithsonian. Here he met Secretary Joseph Henry and became one of the small group of young scientists who lived in the Smithsonian Building and partook of the Henrys' hospitality and encouragement.†

Although Goode kept his official connection with Wesleyan University until 1877, the Fish Commission and the Smithsonian gradually claimed him. He was given the title curator of the museum. In 1876 appeared his first publication under the Smithsonian imprint—*Catalogue of the Fishes of the Bermudas,* a bulletin (No. 5) of the National Museum. This was the beginning of two decades of unremitting hard work for the

† In the old days, during the Joseph Henry–Spencer Baird regime, the hospitality of the Smithsonian Institution extended even to furnishing living quarters for scientific workers it wished to aid and encourage. Especially if the man had no family, he would be allotted one or more of the unused rooms, perhaps in one of the towers, of the then new, brownstone, Gothic-like Smithsonian Building. The geologist Fielding Bradford Meek, for example, lived in a north-tower room from the time he went to Washington in 1858 until his death in 1876. Frank Hamilton Cushing, the eminent ethnologist, joined the Smithsonian staff in 1875 and was assigned to rooms in one of the south towers. His friend George Kennan has provided a firsthand account of the sometimes exciting life in the building. He writes (in the *Medina* (N. Y.) *Tribune,* December 27, 1923): "In 1878 when I went to Washington to enter the service of the Associated Press, Frank was living near the top of the south tower of the Smithsonian building south of Pennsylvania Avenue. As he had two rooms and two beds, he asked me to come and stay there until I should find a boarding place. . . . I stayed with Frank for several months, partly because it was a pleasure to be with him again, and partly because Dr. Bessels, the scientist of the Hall Arctic expedition, also had a room in the building, and he was another interesting character. Frank and I got our dinners at the restaurant on Pennsylvania Avenue widely known to old Washingtonians as 'Harvey's'; but our other meals we took in our rooms. . . .

"The room under ours at that time was the workroom of the well known ornithologist Robert Ridgway. It contained hundreds of skins of birds which had been treated with benzene to protect them from insects, and the floor was covered with fragments of wooden boxes, wrapping paper, excelsior, and other combustible material used in packing. The whole room was as inflammable as a powder magazine. One morning Frank happened to want more heat than he could get over a mere cooking lamp, and he thought he would go down and make a fire in Mr. Ridgway's stove. Not noticing on the floor an open can of benzene which the ornithologist had been using, he struck a match. Instantly the vapor in the air flashed out and set fire to the benzene in the can, which sent up a column of flame four or five feet high. In half a minute the can would have been melted apart and the burning benzene would have run all over the tinder-like stuff on the floor. Frank, with perfect self-possession, lifted the blazing can between his hands, carried it steadily across the room without spilling a drop, and set it down in a solid iron coal-scuttle, where it burned itself out without setting fire to anything else."

It may have been that Cushing's miraculous presence of mind was inspired by stern fire warnings, for it had been but a dozen or so years before that there had occurred a disastrous fire in the Smithsonian Building which destroyed many of James Smithson's personal relics, valuable records and files, and most of the John Mix Stanley collection of Indian paintings.

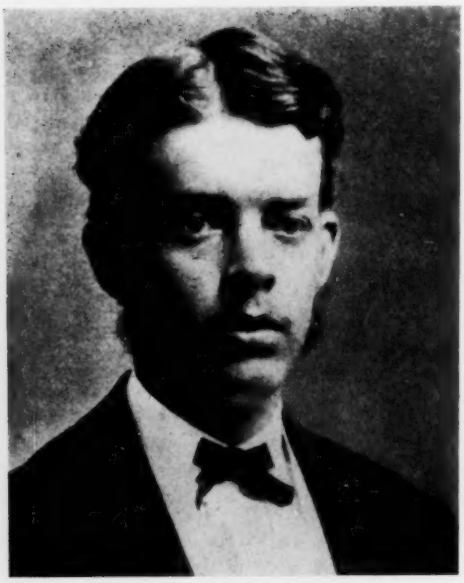
Institution to which he became singularly devoted.

On November 27, 1877, he was married to Sarah Ford Judd, only daughter of Orange Judd and a woman who shared his love of nature. Their marriage was a happy one. As Secretary Langley has recorded, Goode was "eminently a domestic man, finding the highest joys that life brought him with his family and children." This devotion to family extended to a keen interest in genealogy, especially that of the Goode family, and impelled him to work twenty-five years preparing his book *Virginia Cousins,* which, as the title-page painstakingly explains, was "A Study of the Ancestry and Posterity of John Goode of Whitby, a Virginia Colonist of the Seventeenth Century, with notes upon Related Families, a Key to Southern Genealogy and a History of the English Surname Gode, Goad, Goode, or Good from 1148 to 1887." This work, begun as a hobby, turned out to be a fine and model piece of genealogical scholarship. He saw the sociological and historical significance of genealogies, and accordingly was determined that the history of the Goode family should be as nearly perfect as it was in his power to make it. "Every man," he quotes Emerson as saying, "is a bundle of ancestors," and then inserts a special excursus "for the future use of the four little bundles at this moment sheltered beneath his roof-tree," referring to his four children, a daughter and three sons, the eldest then about nine years of age. In Washington the family had a home on Lanier Heights, on a street overlooking the area of the Rock Creek Valley that was then being developed into the National Zoological Park.

One of the first big jobs that Goode undertook after becoming established at the Institution was to help Secretary Baird organize and execute the Smithsonian and Fish Commission exhibit at the Philadelphia Centennial Exposition in 1876. They saw in this Exposition a great opportunity to bring the scientific work of the Federal government to the attention of thousands of people. They wanted to show especially what a great national museum could mean to a nation, culturally as well as scientifically. For this was, in fact, a crucial time for the National Museum. Although the Smithsonian from its very beginning had been delegated to provide for a national museum, Congress had not been overgenerous in appropriating funds for its upkeep. This treatment was particularly unfortunate in view of the great influx of material to be cared for resulting from the United States Exploring Expedition under Captain Charles Wilkes and from the explorations of our Western Territories. Only about $4,000 a year was being appropriated to care for the collections; there was therefore little chance of expansion; and in 1858 Secretary Henry said: "It will, therefore, be the policy of the Institution, unless other means are provided, to confine the collections to illustrations of the products of the North American continent." In 1859 a guidebook was published with the title "Guide to the Smithsonian and National Museum," and about the same time the words "National Museum of the United States" were painted over the door of the exhibition hall. But actually the name "National Museum" had never been given legal sanction by Congress,

GEORGE BROWN GOODE

THIS PICTURE WAS TAKEN WHILE HE WAS ASSISTANT SECRETARY OF THE SMITHSONIAN INSTITUTION.



GEORGE BROWN GOODE

DURING STUDENT DAYS AT WESLEYAN UNIVERSITY.

and this was not to happen until the erection of the new museum building following the close of the Philadelphia Exposition.

The future of the National Museum was close to the hearts of Baird and Goode, and they were quick to take advantage of the publicity afforded by the Centennial Exposition. As Langley has recorded, Baird was—

> determined that the exhibition made by the United States should bear witness to what the Museum could do. . . . The Government made a loan of several millions to the Exposition, which no one then supposed would ever be repaid. Members of the appropriations committee felt quite safe in jokingly assuring Professor Baird that if the money was repaid an appropriation for a National Museum building should not be withheld. The entire staff of the Museum, including several unpaid volunteers, with Goode at their head, gave all their energies for nearly a year to make the Government and especially the Museum exhibit a success, feeling that the future of the Museum was at stake. Individuals all over the country were called upon to assist by advice or material in their special lines. Thousands of letters were written and thousands of exhibits gathered. Here Goode had his first training in the arts of exhibition, in which he finally became the acknowledged master. Many were the discussions as to system, selection of exhibits, causes, labels, and method in general. It was indeed a liberal education to those engaged in the work. . . . The result was a glorious success, acknowledged by all beholders.

The part of the exhibit that Goode directed was unified under the general theme "Animal Resources of the United States." He prepared an elaborate classification and catalogue of all the objects that made up the display. In addition, there was an exhibition of minerals and another of ethnologic materials.

Important, too, was the fact that Goode was kept thinking seriously and deeply on the problem of museum organization and administration, so that when the new museum building was completed in 1881 he was ready with a well-considered plan of what a public museum ought to be. By that time he had been made assistant secretary of the Smithsonian, in direct charge of the United States National Museum, and was recognized as a leading museum authority. And Secretary Baird gave him free rein in developing his ideas. A national museum, he believed, should serve a threefold function. First, it should be *a museum of record,* "in which is preserved the material foundation of an enormous amount of scientific knowledge." Second, it should be *a museum of research,* "which aims to make its contents serve in the highest degree as a stimulus to inquiry and a foundation for scientific investigation." Third, it should be *an educational museum,* "through its policy of illustrating, by specimens, every kind of natural object and every manifestation of human thought and activity." This was the ideal he set up for the National Museum, which he directed.

But he went farther than that. He developed a philosophy of museum administration that was broad and sound and made the conduct of a museum virtually a science in itself. And in this he demonstrated his originality, thoroughness, foresight, and organizing ability that made him a genius in his line. Many of the principles he laid down and the organization plans he codified are still effective today. The secret perhaps was, as it has been in many another man's success, that he believed wholeheartedly in what he was doing—he was certain of the efficacy of the museum as a cultural agency. He summed up his principles in these positive words: "The degree of civilization to which any nation, city, or province has attained is best shown by the character of its public mu-

THE APPROACH TO THE SMITHSONIAN BUILDING IN GOODE'S DAY

seums and the liberality with which they are maintained."

Possibly we need to be reminded that museum exhibition techniques and methods have changed considerably since Goode's day. As Clifford C. Gregg recently wrote in the *Museum News:*

> Our emphasis was then on cataloging and arranging materials, classifying them and describing them. We achieved orderly and, in many cases, attractive displays. Our interpretation was through descriptive labels in our exhibition collections, and through publication of our scientific studies. This procedure was, I believe, in keeping with the times, and in certain instances perhaps ahead of its time. However, times change. The cultural level of the general public and its knowledge within the fields of our operations have increased considerably.

The aims and principles of museums, as formulated by Goode, are still the same. The program is still "research, record, and the dissemination of knowledge."

Goode differed from many museum administrators in being able to bring to bear upon his work a phenomenal breadth of learning and variety of interests. He was a true scholar, one who was not only able to apply himself with distinction to a specialized field (in this instance ichthyology), but also to see the necessity for the integration of all knowledge of whatever kind and from whatever source; and certainly in him the aspirations of James Smithson found a staunch protagonist. To quote Langley again:

> He had a strong interest in literature, and wrote in an excellent English style—clear, direct, and unpretentious. I have never met a mind in touch with more far-away and disconnected points than his, nor one of the same breadth and variety of writing, outside the range of his own specialty. He had fine aesthetic tastes and derived keen enjoyment from everything that was beautiful in nature or in art. He knew all natural sights and sounds, and recognized the note of every bird. . . ."

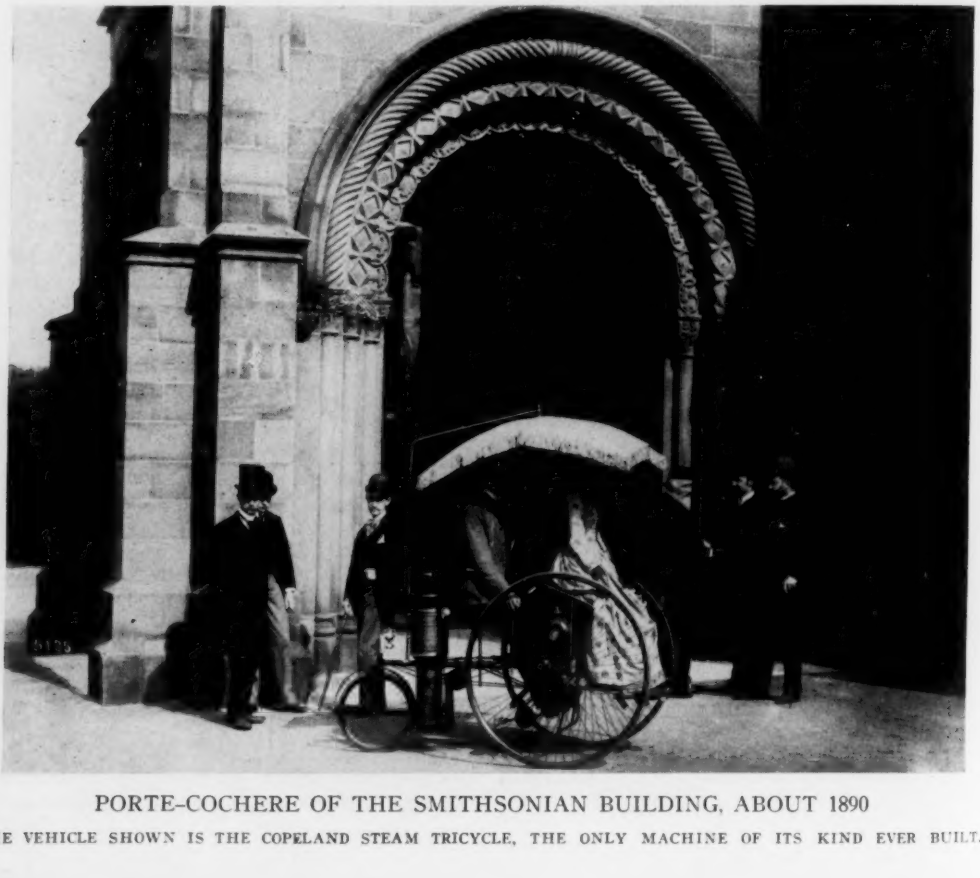
I have talked with men who knew George Brown Goode, and with one accord they have characterized him in the highest terms. They remembered him, after fifty years, as a man of great and generous spirit, of sterling and loyal character. He apparently possessed

that none-too-common ability to make people like him and at the same time to maintain firm control over those under him. "He had a hearty way of meeting associates . . . ," said Theodore N. Gill, a colleague at the Institution, "but in spite of his gentleness, firmness and vigor in action became manifest when occasion called for them."

GOODE'S contributions to science were manifold. His first loyalty in line of research was to the study of fishes, and in spite of his increasing administrative duties he turned out dozens of substantial ichthyological papers, many of them published in the *Proceedings* and *Bulletins* of the National Museum and many of them written in coauthorship with Tarleton Hoffman Bean, who was then the Museum's curator of fishes.‡ The culmination of this collaboration was the publication of *Oceanic Ichthyology,* a treatise on the deep-sea and pelagic fishes of the world, based chiefly on the collections made by the steamers *Blake, Albatross,* and *Fish Hawk* in the northwestern Atlantic. This work comprised two large quarto volumes, which appeared the year of Goode's death, 1896, as a *Special Bulletin* of the National Museum. It was, as David Starr Jordan said, "Goode's most important scientific study" and showed "a distinct advance over all other treatises of abyssal fishes. . . ." In it 156 new species and 55 new genera of fishes belonging to the Atlantic abyssal fauna were named for the first time.

Only once did Goode try his hand at purely popular writing on fishes. This was his book *American Fishes: A Popular Treatise upon the Game and Food Fishes of North America, with Special Reference to Habits and Methods of Capture,* published in 1888 and widely

‡ In Dean's *Bibliography of Fishes* I count 124 titles by Goode alone, 39 by Goode and Bean, and 14 by Goode and others.



PORTE-COCHERE OF THE SMITHSONIAN BUILDING, ABOUT 1890

THE VEHICLE SHOWN IS THE COPELAND STEAM TRICYCLE, THE ONLY MACHINE OF ITS KIND EVER BUILT.

AN OLD PICTURE OF THE BACK OF THE SMITHSONIAN BUILDING

IN THE PENS IN THE CENTER OF THE PICTURE LIVE ANIMALS WERE KEPT BEFORE THE NATIONAL ZOOLOGICAL PARK WAS ESTABLISHED. TODAY THE OPEN AREA OF THIS PICTURE IS OCCUPIED BY SMALL BUILDINGS.

reviewed at home and abroad. In this book he showed himself a literary artist as well as a scientist and succeeded in his expressed ambition to write a book that "people would buy and read" rather than technical reference works, however fundamental, for specialized and esoteric readers.

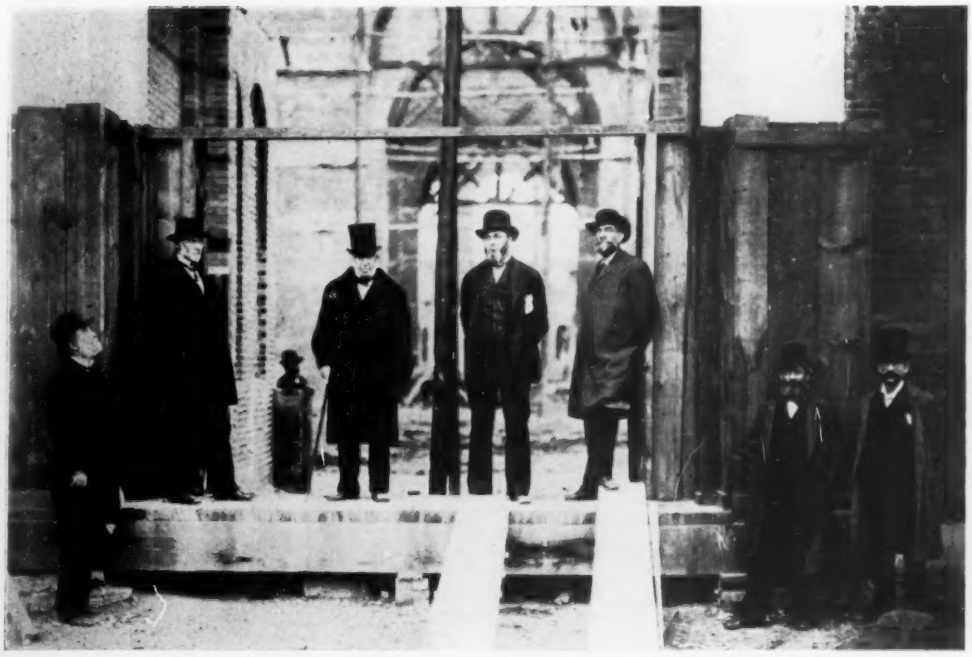
But Goode ultimately perhaps will be remembered as a historian and organizer of science rather than as a scientist. It was well enough, he thought, to catalogue biological specimens, to work over collections and prepare taxonomic treatises, to map the distribution of forms of animal life over the earth, or in other ways to add little by little to the sum total of our knowledge of natural phenomena, but science too needs a synthesis. It must be correlated with the past, and the record of past achievement must be preserved and interpreted for the future. "The history of a science is the palladium of its freedom; it prevents it from being tyrannized over by narrow bigoted viewpoints." Goode would have concurred in this motto, which appeared with a lithographed portrait of Ludwig Choulant in 1842; and for the epigraph of one of his essays he quoted Herbert Spencer: "Is not science a growth? Has not science, too, its embryology? And must not the neglect of its embryology lead to a misunderstanding of the principles of its evolution and of its existing organization?" Goode was the one of his generation of American scientists who saw this most clearly.

On January 12, 1887, Goode was made assistant secretary of the Smithsonian, with virtually full responsibility for the direction of the National Museum; and in the nine brief years that were left to him he was to bring out (in addition to voluminous other material) six or eight papers on the history of science that were to prove classic. The first of these, "The Beginnings of Natural History in America," he had already delivered the previous February as a presidential address before the Biological Society of Washington. Beginning with Thomas Harriot, the earliest English naturalist in

America, he traced the development of American natural science to the end of the Revolutionary War, a period of nearly two hundred years, and put on record a mass of historical data indispensable to later historians of science. In a second paper, "The Beginnings of American Science: The Third Century," delivered before the Biological Society in 1887, he carried forward this history to his own day. The next year he read his essay "Museum-History and Museums of History" before the American Historical Association meeting in Washington, which was followed in 1889 by "The Origin of the National Scientific and Educational Institutions of the United States," presented to the same As-

THE OLD NATIONAL MUSEUM BUILDING

IT WAS BUILT IN 1879 DURING GOODE'S FIRST YEARS WITH THE SMITHSONIAN INSTITUTION.



THE OLD NATIONAL MUSEUM BUILDING DURING CONSTRUCTION

From left to right: GENERAL M. C. MEIGS, U.S.A.; GENERAL WILLIAM T. SHERMAN, SMITHSONIAN REGENT AND CHAIRMAN OF THE BUILDING COMMITTEE; PETER PARKER, REGENT; SPENCER FULLERTON BAIRD, SECRETARY OF THE SMITHSONIAN; ADOLF CLUSS, ARCHITECT; W. J. RHEES AND DANIEL LEECH.

sociation. Finally he published three essays which contained his mature ideas and philosophy with respect to museums—"The Museums of the Future," in 1889; "The Genesis of the United States National Museum," in 1893; and "The Principles of Museum Administration," in 1895.§ After Goode's death, all these papers were collected and reprinted in the *Goode Memorial Volume,* published (in 1901) as Part 2 of the "Report" of the National Museum for 1897. This volume, because of Goode's contributions, is one of the most important ever published on the history of American science.

Goode's devotion to science history manifested itself in one other respect—his unusual interest in bibliography. He knew as well as anyone the drudgery connected with the compilation of indexes and bibliographies, yet he realized that they are the indispensable tools of the research worker and that there is an obligation laid upon every generation of scientists and scholars to provide works of this character. He would have agreed wholeheartedly with Pope's oft-quoted couplet:

> Index-learning turns no student pale
> Yet holds the eel of science by the tail.

As early as 1874 he began collecting materials for "An Index Bibliography of American Ichthyology," in which he planned to "enumerate by title every writing ever published which refers to American fish or fisheries." This was indeed a gigantic undertaking, and he did not live to see it completed.** He inaugurated a series called "Bibliographies of American Naturalists," several of which were published as bulletins of the National Museum. Three of them—

§ L. V. Coleman, director of the American Association of Museums, tells me that this essay has recently been reprinted in South Africa.

** Goode's labors on this index were not wasted, however, for in 1910 when Bashford Dean was compiling his monumental *Bibliography of Fishes* the Smithsonian made Goode's index available to him for incorporation in his work. Dean's *Bibliography* was published by the American Museum of Natural History: Vol. 1 in 1916, Vol. 2 in 1917, and Vol. 3 in 1923.

Photo by T. W. Smillie

THE INDIAN EXHIBIT AT THE PHILADELPHIA CENTENNIAL EXPOSITION OF 1876

bibliographies of Spencer Fullerton Baird, Charles Girard, and Philip Lutley Sclater—appeared under Goode's name, and he had begun work on two others, those of Theodore Nicholas Gill and David Starr Jordan, both renowned ichthyologists. These bibliographies were no mere trifling lists, but were scholarly, annotated, and well indexed. The one of Baird, published in 1883, alone aggregates nearly 400 printed pages and still stands as an invaluable source in the study of the career of the Smithsonian's second secretary.

Goode's interest in history even outside the realm of science was not that of a dilettante. Besides his great book on genealogy, *Virginia Cousins,* already mentioned, he wrote "The Literary Labors of Benjamin Franklin," delivered as an address before the American Philosophical Society. He was a member of the council of the American Historical Association and was instrumental in getting through Congress the Act of its incorporation (1889), under which the Association's reports are published by the United States Government. According to law, these reports are transmitted to Congress through the secretary of the Smithsonian Institution. He held membership in the Virginia Historical Society, the Columbian Historical Society of Washington, and the Southern Historical Society. He helped to organize the Sons of the American Revolution. He assisted also in the organization of the Daughters of the American Revolution and designed their seal. Furthermore, he wrote extensively on the history of the Smithsonian itself, and edited and wrote much of the large volume published the year after his death commemorating the fiftieth anniversary of the founding of the Institution.

All these interests and talents added up to make G. Brown Goode the consummate museum director and a godsend to Secretary

Baird, who was able to place full trust and responsibility in his assistant secretary. Every museum man ought to have the collecting instinct, and Goode had it to a remarkable and refined degree. But he collected always against the background of his broad museum philosophy, that a museum should not be a mass of collections per se, but "a house full of ideas and a nursery of living thought." And it was not only in his own field of natural history that his genius for collecting displayed itself.

> He would [said Langley] bring back from every exposition which he attended methodical collections, frequently overlooked by others . . . not of miscellaneous objects, but of a series which could themselves be placed on exhibition. These might be musical instruments, ecclesiastical art, early printed books, medals, or ivories,

all selected with taste and discrimination. During his regime the number of specimens in the Museum increased from about 200,000 to more than 3,000,000. This increment might well have resulted in chaos had it not been for Goode's organizing ability and his definite aims for his organization.

Honors and responsibilities came to Goode one after another. He served as commissioner for the United States government at the International Fisheries Exhibition in Berlin in 1880 and in London in 1883. He represented the Smithsonian at all the great expositions in this country in the decade from 1884 to 1895 and was a commissioner and for a time acting commissioner-general to the Columbia Exposition at Madrid in 1892.

Goode's method of assembling an exhibition, described by Langley, is revealing:

> The plans of the floor space, the cases, the specimens were all carefully arranged in advance. Boxes were especially made of lumber which could be utilized for cases or platforms. Cases were marked, and not very long before the opening of the exposition the entire mass would be deposited on the bare space assigned to the Smithsonian exhibit. Usually other exhibitors had their material half arranged by this time, and the fear was expressed by sympathetic bystanders that the Smithsonian would not be ready. The cases would be unpacked and the specimens put in them in whatever position they happened to stand, and up to the last day all would seem to be in confusion; but Doctor Goode knew his resources and his men as a general knows his army. Suddenly all detailed work would come to an end, and in the course of a few hours, as if by magic, the entire exhibit would be put in place.

Goode seemed to take great pride and delight in thus making order out of chaos. He never repeated an exhibition. He received many awards, diplomas, and medals at the various expositions, including the Order of Isabella the Catholic from the Spanish government for his work at the Madrid Exposition.

He served as president of the Biological Society and the Philosophical Society of Washington and was a member of the American Philosophical Society, the National Academy of Sciences, and several foreign scientific societies. He was one of the founders in 1888 of the National Geographic Society, which was to grow in fifty years from a small local scientific organization of 200 members to a great national institution of more than a million and a half members. He was elected in 1881 a member of the Cosmos Club of Washington and, like his Smithsonian colleagues Baird and Powell, served a term as its president.

Gradually, however, the burden upon Goode's shoulders became more than one man could bear, and though still a young man he broke under the physical strain. All who knew him have remarked how surely he "burned himself out." On September 6, 1896, he died at his home in Washington, at the age of forty-five years; but into those years had been crowded the work of a dozen ordinary men. With his death a golden age of natural history came to an end. There were, of course, many of the old-time naturalists still alive, but the age of acute specialization in science had set in, and perhaps never again could there be a man like George Brown Goode.

He was buried in Oak Hill Cemetery in Georgetown, D. C., not far from the tomb of Joseph Henry and that of his beloved mentor, Spencer Fullerton Baird. Today his unmarked grave may be found on inquiry from the caretaker; but perhaps he needs no other monument than the solid and enduring part he played in guiding the destiny of the Smithsonian Institution and its ward, the National Museum.

TIME

ADVANCEMENT THROUGH THE YEARS

GEORGE G. ENSIGN

Mr. Ensign, who has had extensive and varied experience in construction and steam-power engineering as well as in precision instrument manufacturing and research, received his Master of Science degree at Iowa State in 1933. He is now director of research for the Elgin National Watch Company.

ANY article about the timing device worn on your wrist or carried in your pocket—whose accuracy more often than not is just taken for granted—must of necessity go back through the centuries to those early scientists whose primary discoveries have been refined and included in the timepiece of today.

Although hardly comparable to watches of the modern era, developments in this field began several hundred years ago. In 1370, the first clock with wheels and pinions was made by a Hollander named Henry de Vick. It was subsequently presented to Charles V, King of France. The clock was a rough, heavy machine driven by a weight of more than five hundred pounds. It was regulated by a bar, which vibrated back and forth in a horizontal plane. There was only one hand on this clock, and it pointed to the hours. The name "clock" itself was taken from the bells, *glocken,* or *cloches,* that were used to strike the hours.

It was not until the sixteenth century that watches, or "pocket clocks," first came into existence. Peter Henlein, a locksmith living in Nuremberg, Germany, is generally credited with making the first watch in 1504. It was spring-driven, made entirely of iron, and stood about six inches high. The famous "Nuremberg Egg" was made about 1550, and many writers have confused this timepiece with the one built by Henlein. There is a difference, however, in that Henlein's timepiece was drum-shaped, whereas it seems likely that the first egg-shaped watches appeared shortly after Henlein's death in 1542. It is interesting to note that these watches sold for about $1,500 in terms of today's currency. The motive power came from a coiled spring. The derivation of the term "watches" seems to come from the fact that night watchmen carried these timepieces when the price was reduced.

Gradual progress was made in improving watches during the next three hundred years. By 1687, watches had become smaller and had acquired minute hands. It took ninety-three more years to achieve second hands. Undoubtedly, the main reason why there were no minute or second hands on timepieces prior to this time was the general inaccuracy of the watches of that period, caused by the fact that lever escapements were not then being used. Watches had no balance spring or hairspring until 1661, when a crude hairspring was manufactured from pig's bristle. Dr. Robert Hooke, of England, made the discovery as a means of applying the pendulum principle to watches. The pig's bristle that he used for this purpose was described as "a tender, straight spring, so affixed that one end thereof plays backward and forward with the balance."

One of the most important advances in watchmaking came in 1704 when Nicholas Facio and Peter and Thomas Debaupre were awarded a patent covering the art of piercing rubies and other precious stones for use as bearings. Another important achievement came in 1740 when Benjamin Huntsman of England developed the crucible steel process for making steel mainsprings. The crucible steel process has since been used by many industries requiring carbon steel of the highest quality.

The watch, considered as an instrument, had reached a stage of high development by

1800. Watches made one hundred and fifty years ago will still compare favorably with many more recently invented instruments in regard to degree of progress. This may be attributed to the interest shown by early scientists such as Hooke, Huygens, Newton, and Airy and the attraction of watchmaking for the most skilled mechanics of that day. The watch was a challenge to these scientists and artisans because it was the most complicated mechanism of that period and required extraordinary workmanship.

The precision required in watchmaking gave the profession the standing of an art, and it has been so considered until the past twenty years. The methods of mass production and the use of interchangeable parts were until recently not applicable to the watch industry. Each watch required individual adjusting and fitting of parts. Through the improvement of gauging techniques and inspection procedures, most modern watch manufacturers have achieved interchangeability of parts, and assembly lines are beginning to appear in the industry. This progress is one of the most significant developments in watchmaking. It is a story in itself, but an idea may be had by considering the problem of the manufacture of more than one hundred tiny parts, which must fit interchangeably into a movement the size of a dime.

NEW MAINSPRING MATERIAL

THESE TWO MAINSPRINGS WERE EXPOSED TO WATER-SATURATED AIR FOR 18 DAYS. THE ONE ON THE LEFT, MADE OF A NEW ALLOY, IS RUST-FREE; THE CARBON STEEL SPRING ON THE RIGHT IS RUINED.

This has not been an attempt to tell the romantic story of watch development through the years, but merely to present a background against which to consider certain modern developments of this industry. Research in the development and improvement of the watch is at present increasing in scope, so that new and better ideas and improvements are appearing at an increased rate and may be expected to continue through the years.

The jeweled wrist and pocket watch as we know it today is composed of well over one hundred parts. Of these, the most important is the hairspring-balance combination, which has often been termed the "heart" of the watch. The development of the first hairspring from pig bristle was considered a radical change when it was announced because, until that time, no device having a definite period of oscillation had been produced for portable timepieces. Before that time, accurate pendulums were used that operated on the principle of gravity, but these were suitable only for stationary clocks. It is easy to see, of course, what a tremendous impetus the development of the hairspring gave to accurate watch performance. If it had not been for this development, the watch would still be a cumbersome, inaccurate instrument.

The modern developments in watch hairsprings and balances may be summarized in several major steps. First, there was Hooke's pig-bristle spring and solid metal balance. Then came the solid balance, with metal hairspring but without temperature compensation. This was unsatisfactory because, as temperatures rose, the balance expanded and became larger and the hairsprings became weaker, so that the watches slowed down. Conversely, when temperatures went down, the balance became smaller and the hairspring stiffer, so that the watches ran faster. Watches, as a result of temperature changes, thus tended to run slower at high temperatures and faster at low temperatures.

The next major step in the evolution of the

PRETIMING A HAIRSPRING–BALANCE ASSEMBLY

IN THIS VIBRATING MACHINE THE LENGTH OF THE HAIRSPRING IS ADJUSTED TO THE MOMENT OF INERTIA OF AN INDIVIDUAL BALANCE SO THAT EACH UNIT WILL KEEP TIME WHEN PLACED IN A WATCH MOVEMENT.

hairspring balance introduced a balance rim composed of bimetallic elements, similar to those used in thermostats. In the bimetallic balance, the metal with the largest coefficient of expansion is placed on the outside, so that with increases of temperature the ends of the rim will be caused to spring inward toward the staff, thus reducing its moment of inertia and compensating for the loss of stiffness of the hairspring.

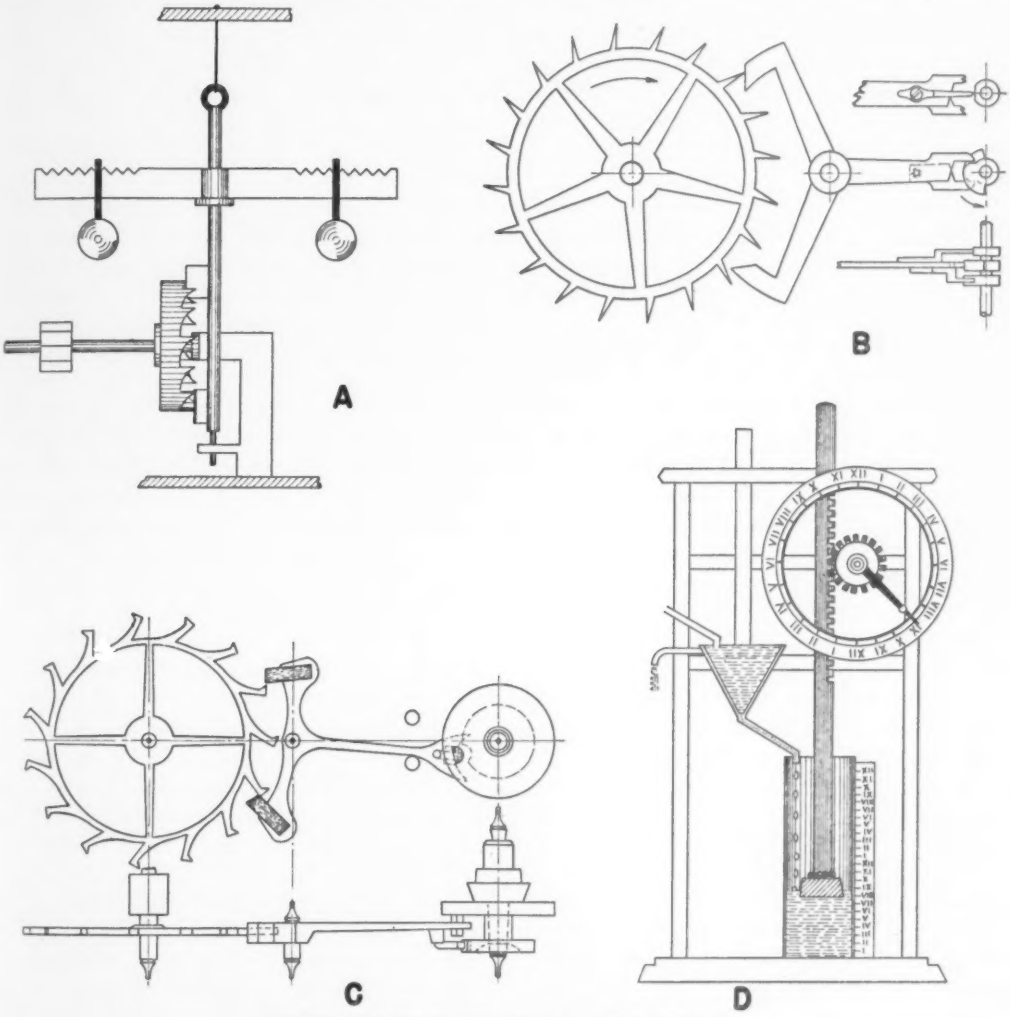
The first bimetallic elements to be used were brass and steel, with later developments substituting invar for steel. Invar was developed in 1896 by Dr. C. E. Guillaume, director of the Bureau International des Poids et Mesures, in Paris. Dr. Guillaume developed his metal to provide a standard measure for the meter that would not expand or contract upon exposure to normal changes in atmospheric temperatures. Its basic composition is 36 percent nickel and 64 percent iron.

In 1913, Dr. Guillaume developed elinvar, the basis of practically all modern compensating hairsprings manufactured today. Elinvar is a refinement of invar, as are Elginite, Nivarox, and other alloys currently used. Some of these variations include heat-treatable materials, thus producing harder and more resilient hairsprings. These materials are characterized by their peculiar ability to become more stiff with an increase of temperature rather than to weaken as does steel.

It is interesting to note that the bimetallic balance was designed to compensate the temperature-stiffness change inherent in the old type hairspring. The modern compensating alloy spring can now be so controlled as to reverse this relationship, and the hairspring can be made to compensate for the change in balance moment. This permits a stability and a simplicity in the modern watch not previously attainable.

A further modern refinement of a compensating balance was recently introduced in the United States in the form of a compensating hairspring and a rigid balance consisting of a stainless steel rim and an invar crossarm. With changes of temperature, this balance will distort to a slightly elliptical shape. This phenomenon, in conjunction with movable weights on the rim, is used to provide an extremely sensitive adjustment of the temperature compensation of a watch in a manner not heretofore possible.

Development of the hairspring and balance to their modern forms now permits watch manufacturers to pretime, or "vibrate," their assembly to an accurate timekeeping rate prior to its installation in the watch. An electronically controlled machine recently developed compares the frequency of vibration of the balance-and-hairspring assembly to an



DEVELOPMENT OF THE ESCAPEMENT

(*A*) THE EARLY "VERGE" ESCAPEMENT RUNNING AN OSCILLATING MECHANISM KNOWN AS THE "FOLIOT." THERE WAS NO HAIRSPRING, AND THE ESCAPEMENT WAS ALWAYS IN CONTACT WITH THE OSCILLATING MEMBER. (*B*) THE FIRST DETACHED LEVER ESCAPEMENT, INVENTED BY THOMAS MUDGE. THIS MECHANISM PERMITS A FREE SWING OF THE BALANCE WITHOUT CONTACTING THE ESCAPEMENT EXCEPT DURING A VERY SHORT INTERVAL. (*C*) A MODERN LEVER ESCAPEMENT. (*D*) AN EARLY WATER CLOCK, THE FUNCTION OF THE ESCAPEMENT AND BALANCE BEING PERFORMED BY DROPS OF FLOWING WATER.

electronic frequency standard. Simultaneously, it adjusts and establishes the length of the hairspring to match the moment of inertia of the balance at the frequency required to keep correct time.

After the pretimed balance assembly is in operation in a watch, another modern electronic machine—the rate recorder, or timer—"listens" to the tick rate by means of a microphone and compares it to a standard frequency. This machine permits a watch manufacturer or repairman to determine and adjust the timekeeping accuracy of a watch in a matter of minutes as contrasted with many days of running on the adjusting racks so indispensable only a few years ago.

LUBRICATION has for centuries been a perplexing problem for the watch manufacturer and repairman. Progress in solving this problem has been rapid within the past few years.

A good watch lubricant must remain fixed in the spot where it is applied. If the lubricant has spreading qualities, it will not stay in the bearings and may even spread to parts of the watch that must be kept free from oil, and thus affect timekeeping values. A watch lubricant must also be nonoxidizing; otherwise, it will become gummy or sticky and hinder rather than help the mechanism to function. Its viscosity must be within usable limits over a wide range of temperatures. Further, a good watch lubricant must not be too sensitive to surface films that may deposit on the bearings. Finally, it must be "oily"—an excellent antifriction agent.

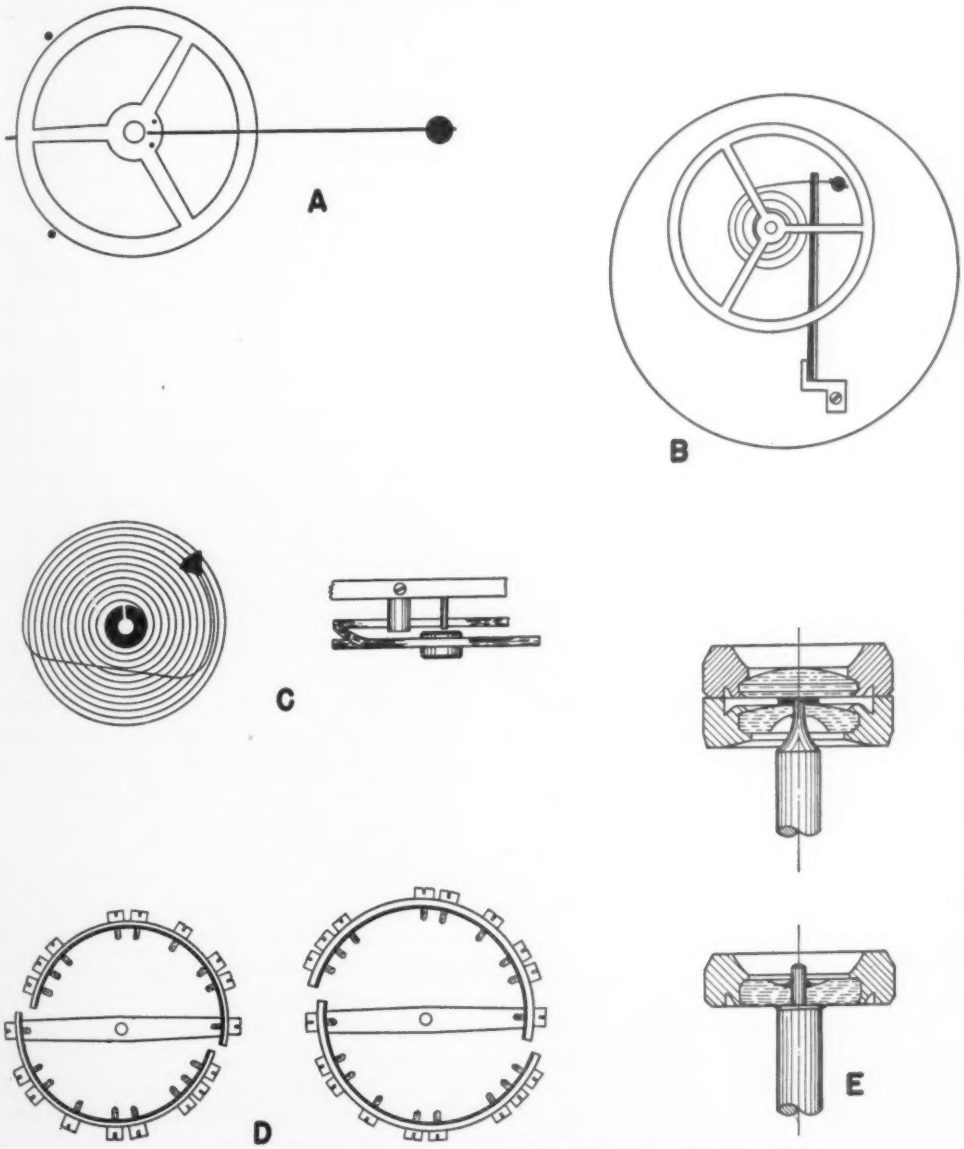
In the early days of large-scale watch manufacture, it was found that the best watch oil was obtained from the porpoise, an aquatic mammal brought back by the whaling ships. The jaw of the porpoise has a small gland containing oil, which, when refined, provided most of the necessary qualities of a good watch lubricant. At one time it was used almost universally. The most serious defect of porpoise-jaw oil was that, upon exposure to light, it rapidly became acid and caused metal parts to corrode.

In the past, practically all kinds of natural oils have been tested and rejected. Mineral oils were tried first and then animal and vegetable oils, such as neat's-foot, walnut, and castor. In many cases, these oils had fine nonspreading qualities, but invariably they were easily oxidized and thus became gummy or sticky. Through the 1920s the only successful forward strides were in refining natural oils so that the acid condition was minimized. Mineral and other oils were blended to obtain better nonspread oils. A German oil was manufactured from an animal oil, such as neat's-foot, blended with mineral oil. This was unsatisfactory because it could not be controlled. The qualities of one batch might be good, the next unsatisfactory. This was true in general of all the natural oils, for they varied from batch to batch and from season to season. In the early 1930s, the French developed a series of oils comparable to the German product in that they were an animal oil blended with mineral oil for proper viscosity. To these was added a combination of organic dyes to absorb the ultraviolet rays of sunlight, thereby reducing their tendency to become rancid. As these oils were of the spreading type, a product called epilame was developed simultaneously to be furnished with each bottle of oil. The epilame was placed on all watch parts that were to receive the oil, providing a very thin film on which the oil would remain without spreading. This was more or less unsatisfactory because, prior to oiling, one could not be sure a sufficient film was present; also, the film was removed each time the instrument was cleaned.

By 1938, most commercial oils had been tested and found unsatisfactory as watch lubricants. The Elgin National Watch Company even sent one of its research scientists to Europe on an extended search to determine whether any oil or oils had been overlooked. At that time the large oil companies were uninterested in doing large-scale research for an industry that used, at the most, only 40 gallons of oil a year. Finally, Elgin referred the problem to the Mellon Institute for study in cooperation with the Elgin Research Department. In 1940, a synthetic lubricant was developed that had the desired qualities. It was given the laboratory designation of M–56a and was marketed under that name.

In 1943, however, Mellon Institute and Elgin developed another oil even more suitable for the commercial trade. This is called M–56b. It is far less sensitive to bearing surface conditions. It therefore gives satisfactory service with far less rigid cleaning standards. M–56b oil will function satisfactorily at minus zero temperatures and will remain effective in unusually hot climates. This oil is now being widely used throughout the watch industry.

A recent development of great importance



VARIOUS PARTS OF EARLY AND MODERN WATCHES

(*A*) EARLY PIG-BRISTLE TYPE OF HAIRSPRING. (*B*) EARLY TEMPERATURE-COMPENSATING DEVICE. DISTORTION OF THE BIMETALLIC ELEMENT SHORTENED OR LENGTHENED THE EFFECTIVE LENGTH OF THE HAIRSPRING TO COMPENSATE EXPANSION OR CONTRACTION OF THE BALANCE. (*C*) MODERN HAIRSPRING WITH "OVERCOIL" TO ENSURE CONCENTRIC WINDING AND UNWINDING. (*D*) EXAGGERATED DISTORTION OF A BIMETALLIC BALANCE AT HIGH AND LOW TEMPERATURES. (*E*) PROPER LUBRICATION OF JEWELED WATCH BEARINGS.

to the watch industry is a highly corrosion-resistant material for mainsprings. This alloy—called Elgiloy—is a result of twelve years of research at Battelle Institute as an Elgin Watch Company project. It has long been a known fact that the major cause of mainspring failure has been the rust that formed on the almost universally used carbon steel, but none of the stainless materials have had mechanical properties that would allow their use as mainsprings without greatly increasing the spring thickness and spring barrel diameter to obtain the necessary torque, or power. Because of style requirements, space is of major importance. A mainspring of Elgiloy not only furnishes power equal to carbon steel, but slightly exceeds it in strength, thickness for thickness. Further, it is not sensitive to surface conditions that cause fatigue failures in carbon steel.

Still another advantage of the new mainspring is illustrated by its name—DuraPower. This spring will deliver the same amount of power after long periods of use as it delivered when first installed. This nearly perfect stability of the new alloy spring allows a watch manufacturer to adjust his watches more closely before they leave the factory, and the constant power source keeps the rate of the watch regular for longer periods.

In contrast, carbon steel begins to lose power as soon as it is wound into a barrel and within a week or two may deliver only 90 percent of its original power. This loss of power, or "set," is easily understood when it is realized that the outer fibers of the mainspring for the average wrist watch are elongated about 0.035 inches per inch when the watch is fully wound. This, of course, places a stress on the material that greatly exceeds the elastic limit of steel, causing a large permanent deformation. Because of its higher yield strength, Elgiloy does not permanently deform to an appreciable degree when subjected to a similar stress.

The developments reviewed above cover only certain innovations in watch technology. No attempt has been made to discuss the many refinements in watch design contributed through constant research and engineering directed toward making a finer product. These improvements are of primary interest only to those who have a special knowledge of the watch mechanism, but it is hoped that the omission of such material will not create an impression that such work is of minor importance. The basic principles of the watch mechanism have not been changed since the introduction of the lever escapement in 1755. The refinements are so numerous, however, that it requires considerable ingenuity to conceive a new one that will afford distinct improvement. It is quite a tribute to the watch as a mechanism that it has withstood the efforts of engineers, watchmakers, hobbyists, and scientists to introduce a new basic principle, such as a mechanism to replace the balance and hairspring. However, anyone inclined to accept this challenge is urged to pause and reflect upon the problem before him. He has chosen to improve an instrument having an error of less than 0.01 percent and representing the best efforts of many great minds and the most skilled mechanics.

TEAMWORK IN INDUSTRY

E. WIGHT BAKKE

Professor Bakke is Sterling professor of economics at Yale and director of the Labor and Management Center recently established there to conduct an extensive program of research in relations between management and unions and workers. He has studied labor problems in the United States, England, and Scandinavia.

ONE of the most respected of the men who founded this nation once said, "We must all hang together, or assuredly we shall all hang separately." That truth has struck responsible and thoughtful leaders of management and labor in recent years with tremendous force. It is not a new idea. Every successful leader of industry and of the labor movement has been aware of it. The typical unit of work and living is not the individual but the group. The products of effort are social products. "No man liveth unto himself."

Good teamwork in industry and unions in the past has depended in the main on the presence in the organization of some leader of men who from experience or sound intuition sensed the basic laws of human relations and understood how to interest men in the welfare of the group as a whole and the organization that bound them together. He knew how to shape and manipulate the bonds that welded a crowd of varied individuals into a working team.

If the cases of successful development of teamwork are to multiply rapidly enough to avoid a breakdown or explosion within our free-enterprise system, the principles of effective human relations will have to be learned by a lot of executives in industry and labor unions who are not "natural-born leaders." And even a lot of natural-born leaders will have to learn how to use principles developed from experience in small, intimate, face-to-face industrial operations in large, complex, and impersonal organizations. For the main reason so many managers and union leaders are concerned about teamwork today is that their organizations have grown beyond the point where they can exercise their leadership through personal contacts with every member of the team. The force of their personal character and know-how has to be channeled through the medium of other persons and the organizational structures of their companies and unions. By the time it reaches the average worker, that character and know-how appear quite different than it was at the point of origin. Even the natural-born leader faces this problem; and the plain fact is that there are not enough natural-born leaders whose personal power is sufficient for the task to go around.

If the rest of us are to be successful in building good teams, we have to learn the principles of teamwork. We have to learn to organize men. We have to learn the nature of the organizational devices that provide the structure within which men work together as a group. We have to learn what makes these devices effective in promoting the purpose of the enterprise and efficient in satisfying the desires of all those who participate.

One may be fortunate enough to work in close contact with a natural-born successful leader who can give personal tutoring on the subject. If not, he may find some profit in considering some of the conclusions derived from a study of both successful and unsuccessful experiences in industry. In either case, it may prove helpful to have a systematic way of analyzing the problems. For the problems change, and the rule-of-experience principles developed by leaders in the past may not be as adequate in dealing with the problems in human relations we now face as they were in dealing with the problems from which they gained their experience.

Now, assuming we are not satisfied simply to talk about teamwork, but want to get down to fundamentals, how shall we go about it?

I suggest we start at the beginning with the most basic question, What makes a team out of a crowd of individuals? Here they are in a plant or in a union. They've come from all sorts of social, political, religious, and economic backgrounds. They've been brought up in different ways. Some are polite, considerate, and respectful; others are swashbuckling, bigoted pirates, born a hundred and thirty years too late. Some are skilled, competent chaps who know what to do and how to do it; others are fumbling fellows with ten thumbs on their hands and solid bone for a brain. Some of them are natural cooperators; others make the sparks fly whenever they come within sight of another person. The job is to take this bunch of varied individuals and weld them into a team. What are the basic organizational devices one can use? I'm going to call these organizational devices the *bonds of organization.*

The bonds are the same whether the team is a public utility, a manufacturing enterprise, a family, a union, a research organization, a navy, an army, a nation, or a football squad. What are they? They are (1) job specifications and requirements, (2) the communication system, (3) the status system, (4) the system of rewards and punishments, (5) the organizational charter. I shall illustrate each one by reference to a football team; but their relevance to any organization is clear.

NUMBER one is *job specifications.* Every player has a position, the duties of which are well defined. If he is an end or a center or a quarterback, he knows what is expected of him as an individual and as a member of the team. He has a definite job to do. Likewise in a company or union there are job specifications for everyone who participates. Sometimes these specifications are spelled out in black and white. Sometimes they are just understood. But unless members of the team have clear definitions of their jobs, and the other members have the same understanding, the first requisite for good teamwork is absent. How well do workers or union members understand what is expected of them? How well do they understand what is expected of the chief engineer or the president or the union officers? How accurate is the conception of the president or general manager of the job requirements of the pipe fitter or the steam boiler engineer? The first requirement for good teamwork is that each shall understand clearly and adequately his own job specifications and shall have a sufficient understanding of the essentials of the jobs of other people with whom he must work so that he can sense what effect those requirements have on their behavior. Managers frequently say, with good reason, that union leaders don't know what a manager is up against. Do managers know what a union leader is up against as a manager of a union, what his job requirements make it necessary for him to do and say? Do union leaders know what being a manager requires of the person who fills that job?

The second bond of organization that binds a group of individuals into a team is the *communication system.* On a football team there are certain signals that have to be given so that each member of the team knows what he is to do and what the others are doing. There is a method of communication, perhaps a huddle. Someone is responsible for barking the signals. Someone calls time out for a conference. Skull sessions are called to make sure that everyone shares the same understanding of the plays and has possession of the information that makes them willing and able to work together. Every organization has its communication system. Policies, procedures, administrative orders, have to be transmitted down the line. Reactions, performance reports, and many other details have to be transmitted up the line. Do the communications from both directions get through clearly and adequately to the people for whom they are intended?

The communication system has to handle every kind of information that people in the organization want to get across to one another. Much attention has been paid by management to the forms and procedures for transmitting *orders* down the line. Less attention has been paid to procedures for transmitting equally important information concerning the objectives, achievements, and problems of the company down the line, and

for getting into the hands and minds of management the problems of the people they supervise. Unions have also frequently neglected the line of communication *from* the rank and file. The communication system has to carry information in both directions.

How frequently do managers or union leaders sit down and ask themselves these questions: What information must I have and what information must those under me have in order to work together effectively? What instruments have I devised for making that information available? Is the system adequate, and does it place in the hands of every person clear and concise information that he needs to do his job and to maintain satisfactory relations with the rest of the team?

The third bond of organization is the *status system*. Every team has a captain whose word is supposed to be law, whose approval is required at critical times. There are coaches and the head coach. Sometimes there are players on the team who have no official status but to whom others look for direction. They "rate," not formally, but informally. In every organization men are placed in a status system, and each place carries its authority over, and obligations to, others, its deference from, or to, others. Every company and union has an organizational chart that is supposed to represent this hierarchy of authority and deference. Our studies have indicated that this organizational chart frequently bears little resemblance to the actual hierarchy of authority and deference in the company or union. Good teamwork is a process governed by the status system as it is, not as it appears on the chart in the president's office. If the formal status system is inconsistent with the informal status system, teamwork is bound to suffer.

The fourth bond of organization that holds a group together is the *system of rewards and punishments*—rewards for satisfactory work, punishments for unsatisfactory work. On a football squad rewards may be formal, like a letter, or bars for additional service; or they may be as informal as a cheer for a good play or a slap on the back and a resounding "Atta-boy, Mac." Punishment may also be formal, like being "benched"; or informal, like hard looks, or another player saying, "What are you trying to do, play the whole game yourself?"

In any organization, what is the actual system of rewards and punishments as seen through the eyes of participants at each status level? What do they think are the conditions upon which their achievements and benefits within the organization depend? Are executives assuming that the system is limited to increases and decreases in pay or position? Do they think people look to the organization as one that provides them simply with a pay check and a title, or can the organization provide or withhold other benefits men want in order to live a more complete life?

The system of rewards and punishments is one of the most important group-making devices for which managers and leaders are responsible; but in order to be effective, this system must be realistic from the point of view of the people who are rewarded or punished. My first lesson in the fact that what may appear as a reward for one person may be a punishment for another was when I learned that many workers would consider advancement to a foremanship an unwelcome promotion, and that the receipt of abnormally high earnings from piecework might declass a man instead of giving him superior status with his fellows.

How much do managers, responsible for shaping the nature of rewards and punishments in a firm, actually know about what their subordinates and employees consider to be rewarding and punishing? Have they unconsciously assumed that what has proved rewarding to them must be rewarding to everyone? For instance, consider the pension system. Executives have frequently been with the company a long time and expect to stay with it until they retire. Is the pension system realistically rewarding to those younger employees who may work for half a dozen companies before they are through? Or consider increases in individual pay and promotion. Success as a manager has followed the pattern of winning out over other individuals. Increases in salary and

position are symbols to them of that kind of success. But suppose one's whole experience had taught him that success depended on group solidarity, and sticking together, and sharing bad as well as good fortune. Would a promotion in pay or position that sets one apart from his fellows be a reward?

How broad is the conception of union leaders of the rewards for being a union member? Have they been satisfied to assume that the traditional rewards are the only ones members would like to receive?

I am not offering any dogmatic conclusions here. I am simply suggesting that the system of rewards and punishments leaders work out for their teams will be effective to the degree that it gives people what *they* want if they do well and withholds from them what *they* want if they do poorly, and that a leader has to find out what *they* want by looking at the situation through their eyes, not his own.

The final bond of organization that welds a group of people together is what we may label as the *organizational charter*. It is an intangible but real thing. It is the picture that members of the team have of the team as a whole. That picture is composed of their conception of its purposes, achievements, traditions, and the symbols of these. It is the picture men have of the company when they say, "I work for National Tube." It is the picture men have of their family when they say I am a MacDonald, or a Cabot, or a Koussevitsky, or a Knudson, or a Callahan. It is the picture that men have of their nation when they say, "I am an American." It is the conception they have of their union when they say, "I am a member of the United Steel Workers of America." One of the greatest differences we have found between management and men, or union leaders and members, is their picture of this organizational charter of the firm for which they work, or the union of which they are a part. If the pictures in their minds are too inconsistent, effective teamwork is impossible. What attempts have managers and union leaders made to learn what their employees or members think of the organization as a whole, and whether things can be done to make their picture more consistent with that of the leaders, and to make both the pictures more consistent with the truth? In so doing, they would be concerning themselves with one of the most important bonds of organization that make a team out of a miscellaneous collection of individuals.

For organizers and leaders of a team here are five organizational devices, five bonds of organization that weld the people who work for or with them into a team: job specifications, the communication system, the status system, the system of rewards and punishments, the organizational charter. Their first job is to know the facts about these devices as they are actually operating in their organization.

I have learned as the director of a research team of forty people that I cannot take anything for granted. I cannot assume that these devices in our organization are what I planned them to be or that they appear the same to the people who are working with me as they appear to me. I am busy doing my job. I have my specific duties to perform and my specific obligations to meet; and they keep me at work long after the members of the team have left the office and gone off to attend to their personal affairs. But I know I shall never succeed in organizing an efficient team unless I make these organizational devices satisfying from their point of view. If I were a poet or an artist, or a concert pianist, or someone else who worked independently, I would not have to worry about such things. But I am not. I am the organizer of a team. My success is dependent on the work and loyalty of the members of a team. So the first question I have to ask is what the organizational devices look like through the eyes of the members of the team.

But that is only the first question. The second one is equally important. It is this: Are these devices or bonds satisfying to them? Are they helping them to get from their work what they want to get from it? What are their standards of successful living against which I can check the efficiency of those devices? Again, I say, it is their standards, not mine, that are important, simply because it is their response and not

mine that is involved in getting the job done. If the organizational devices help them to make progress toward their goals, they will put their shoulders to the wheel and give their best. If not, they will hold back, lose their interest, and in so doing, act like a crowd of individuals instead of working together to accomplish the team's objectives.

I AM no authority on the motivations of men; but at the Labor and Management Center we are trying to learn what these are. Whether our investigations have been focused on the behavior of unemployed men, workers who were being organized, relations between union leaders and management, union leaders and union members, or management and employees, we have tried to find out what really made these people tick. What were *their* goals, and how did the environment created for them by the community, the managers, or the union leaders help them or hinder them in their progress toward these goals? Without claiming that the list is complete, or that it has been adequately tested, it may prove of value to list them. As they stand, they are our honest attempts to summarize the responses men make to such questions as "How do you define the difference between a fellow who has got ahead and one who is a failure?" "What do you want your children to get out of life that you have failed to get?" "Is there anyone you would like to change places with? Why?" and similar questions. What are the goals, the standards of successful living, that stand out in the answers to questions such as these? I am convinced that these are major standards against which members of the team measure the adequacy of the organizational devices, the bonds of organization we have been discussing.

Men want to experience *progress* toward, and *security* with respect to, the following goals. And they want to feel that the amount of their progress and security are *just* in the light of their conception of their own worth.

1. *The respect of their fellows.* This is the desire to play a role that the people they associate with consider important and respectable.
2. *Creature sufficiency.* This is the desire to have the amount and quality of food, clothes, shelter, health, etc., and the means to provide them, enjoyed by the most favored of their customary associates.
3. *Increasing control over their own affairs.* This is the desire to have their own decisions and actions effective in shaping the course of their own lives and to reduce the control exercised by others.
4. *Understanding.* This is the desire to have a clear picture of the facts and forces that are at work in their world, the relation between what happens and what caused it to happen—in other words, "to know the score."
5. *Capacity performance.* This is the desire to have the chance to use the full range of one's abilities. It is not the desire to burn one's self out on a particular task. The question as it applies to industrial relations is whether a man's job gives him the opportunity to do the sort of things he thinks he is capable of doing, or whether it calls out only an insignificant portion of his capabilities.
6. *Integrity.* This goal is the hardest to define, but its importance is evident. We use it in the sense of wholeness. A man wants to feel whole within himself; that is, he wants to feel that his actions and principles are consistent. (Call that aspect of integrity, self-respect.) But he also wants to feel whole with the world of people and things about him. He wants to be geared in, to sense that he is a significant part of it all. (Call that aspect of integrity, relationship.)

The second requirement of good teamwork is that the bonds of organization that men experience in the group shall be efficient instruments for helping the members of the team satisfy these desires they have.

I would like to suggest that when a manager or a union leader is found who has been successful in getting good teamwork results, the first two secrets of his success will prove to be these: He was skilled in developing workable organizational devices of the sort we have been talking about; i.e., he knew how to construct effective bonds of organization. Moreover, he had a good understanding of what these goals we have been discussing meant to his people and saw to it that the bonds of organization were efficient in giving them a sense of security and progress and justice toward those goals.

I HAVE simply outlined the framework of devices by which teamwork is carried on,

and the standards of successful living by reference to which the members of the team judge those devices as satisfactory or not. The principles of the relationship between the two would fill many volumes.

I can suggest only a very few of these principles. I shall state them negatively, that is, as inadequacies in these bonds of organization that have turned up in some of our studies. I suspect that many, if not all, of them will be familiar to most readers. At least I hope so, because experience of practical organizers in the past, present, and future is the first testing ground for such conclusions. The question then is: Where we have found evidence of poor teamwork, what was wrong with these organizational devices as measured against the standards of successful living of the members of the team?

Consider, first, inadequacies in the *communication system.*

1. The communication system did not carry down the line all the kinds of information needed and desired. Particularly scanty was information about the purposes, traditions, and problems of the company or union itself, the general economic and social factors that affect the welfare of the company and union, and with which top management or leadership was wrestling. Frequently lacking was clear information about major policies of the company in its relation to the public, to the union, and even to its own employees, or about the major policies of the union in its relation to the public, the company, or its members.
2. The communication system did not carry messages both ways. No careful provision was made to get reactions from, and an understanding of, the problems of men down the line into the minds of management and leadership up the line. I am not speaking of grievances alone. Often even more important than grievances are the day-to-day reactions of men to the company, their jobs, their management, their union officers, their suggestions for improving conditions and processes of work, or the conduct of union affairs, and just the plain facts about what life and work look like from where they stand.
3. The communication line was not short and direct enough—in both directions—so that at the end of the line the same message came out that was started. As a result, short cuts and by-passes led to misunderstandings and situations which men interpreted as undermining their authority.
4. The authority of those who were qualified to originate or to pass on information was not defined clearly and precisely enough so that men could have secure confidence either in the people from whom it came or the information they received.
5. The communications were not designed to motivate emotional acceptance of the information as well as to assure its intellectual comprehension.

Notice in these examples that the communication system was not adequate to give men a sense of security with respect to realizing their goals of understanding, social respect, control, and integrity.

Now turn to the *status system.* What inadequacies in that bond of organization frequently retarded good teamwork?

1. Sometimes men felt they were responsible to, and must look for guidance and formal approval from, more than one boss or official. The lines of authority were confused.
2. The system was so autocratic that only those at the very top could get any real sense of importance from the positions they occupied.
3. Qualifications for filling the higher positions in the status system were so arbitrary or unrelated to effective performance that men's sense of justice was violated.

Notice in these examples that the status system was not adequate to give men a sense of security and justice with respect to realizing their goals of control over their own affairs, social respect, understanding, and integrity.

Now consider the possible inadequacies in the *organizational charter.* In fifteen years of interviewing workers and management in England, Scandinavia, and the United States, I have never found one who did not want to work for a firm or be a member of a union he could be proud of. In every community there exists in the minds of the workers a pretty clear understanding of the firms and unions that "rate" and those that do not. They are arranged in a more or less well-defined hierarchy from "bum" to "swell." Those that rate high in the scale of course have an easier job in developing and maintaining an organizational charter in the minds of the members of the team, which helps greatly to promote good teamwork. But every firm and union must make the best of what it has and try to create a common pic-

ture in the minds of its members of its purposes, achievements, and traditions. The men will develop their own picture anyway. It is the job of leaders to see that every possible feature of that charter that will make men proud of their connection is made vivid and clear. One of the greatest shortcomings of the leadership of companies and unions we have noticed, where poor teamwork was evident, was their failure to pay any attention at all to this requirement for effective group activity. A second shortcoming was their bragging about features or characteristics of the organization that had no basis in fact. The result of these shortcomings of management and leadership is a failure to satisfy the desire of team members for respect and a sense of relationship, or integrity.

Managers and union leaders have given more attention to the bond of organization I have labeled *job specifications* than to any other. That bond has struck them as essential and important. Some of the major inadequacies that do appear, however, are these:

1. They focus entirely on instructions as to how to do a particular job and neglect the definition of how that job is related to other jobs and how the people doing them are related to each other functionally.
2. They leave too little freedom for the initiative and inventiveness of the person assigned to the job.
3. The job specifications are not matched with the actual and potential capacities of the job holders.
4. They are changed so frequently and arbitrarily that people do not know where they stand, and are constantly having to adapt themselves to change. As soon as they get into the swing and rhythm of an operation, it is upset by new instructions.

Such inadequacies keep men from realizing their desire for security, control over their own affairs, and satisfying relationships or integrity.

Finally, we turn to the inadequacies in the *system of rewards and punishments*. The listing of specific shortcomings that have appeared in our studies would fill a book. Let me, therefore, set down only six conclusions about the general nature of these inadequacies.

1. The system that defines what kind of performance results in what reward or punishment is not clearly understood by all concerned. What managers and union leaders think the system is frequently bears very little resemblance to the ideas workers have of it.
2. The rewards and punishments are dispensed arbitrarily and sporadically rather than systematically. In other words, they are not dependable. They seem to be administered by whim rather than by plan.
3. Discrepancies in performance requirements and responsibilities exist among those receiving the same pay.
4. Rewards and punishments are geared solely to individual performance, emphasizing the significance of competition with, rather than cooperation among, men. Man is set against man rather than geared into a team. Rewards are for individual achievement rather than for team accomplishments.
5. The emphasis in the entire system is upon economic rewards and deprivations, ignoring the desire of men for social respect, freedom, capacity performance, self-respect, and a significant relation to people and the world about them.
6. The most important shortcoming I have already mentioned. The rewards and punishments are not realistically set up to jibe with what the people affected consider to be rewarding and punishing.

Wherever these shortcomings are found in the bonds of organization, one or more of the basic goals for which men are consciously or unconsciously striving is moved beyond the horizon. Men get lost in the thickets and underbrush. They lose the sense of satisfying contact and team play with other members of the organization toward a common objective. If they do not adopt a "What-the-hell" attitude, they at least get used to taking the next obvious step without enthusiasm or hope. Obsessed by personal frustration, they go doggedly along their own way unconcerned about the welfare of the whole team. Why should they exhibit a team loyalty if the managers and leaders of the team fail to bind them together with the bonds of organization that provide them with opportunities for, rather than obstacles to, the realization of their standards of successful living?

Does it look like a mountainous task to develop skill in constructing these bonds of organization, and an understanding of the

standards of successful living by which members of the team judge them adequate or inadequate? Of course it is. The organizer of a team is faced with the most complicated set of facts in the world, the nature of human beings and the ways in which they interact with other human beings. The question, Why do men act as they do? is the hardest question in the world to answer. But the answer we must have, and have it right, if we are to organize effective teams.

Managers have faced engineering problems nearly as difficult. If they were to give this problem as much systematic attention and study as they give to the quality of materials, the layout of the plant, the performance of equipment, or the development of a rate schedule, it might not prove insurmountable after all. But it does take systematic attention and study. It cannot be left to guesswork or chance any more than engineering problems can be left to guesswork and chance.

Engineering problems do not have to be left to chance, because systematic ways are available for analyzing these problems. Scientists—physicists, and chemists—provide basic principles and laws as to the behavior of materials and equipment. Schools are established in which men can be trained on the basis of that science. Nothing less than that is required for successful human engineering. The assembling and organizing and directing of a team of human beings are no less difficult than assembling and operating a turbine.

At the moment we could coast for awhile, if necessary, with respect to our knowledge and skill in the field of technology. No major breakdown impends, no irreparable disaster threatens us. But no honest person can survey the conflicts and confusion that characterize our efforts in developing satisfying and productive human relations in industry and avoid the conclusion that a breakdown or explosion is not only possible, but probable, unless we discover and apply better principles of teamwork than we have used to date.

The problem is not alone one of making company and union teams more effective; it is one of making our free-enterprise economy and our democratic society strong in relation to other economies and societies whose principles of teamwork differ from our own. It was never more true than today that unless we all learn to hang together we shall all hang separately. It is one thing to recognize this truth, but it is another to learn how to hang together, and to get from individuals the willing and enthusiastic activity and loyalty that makes it possible to hang together. That is the task to which the organizers and leaders of teams in America are called. It is the task that those of us in the universities are trying to make easier by the development of a science of human relations that practical men can use. Together we carry on our shoulders the responsibility for the success or failure of this task. Our failure will carry the gravest consequences for the welfare of our people, their institutions, and their way of living. Our success will uncover fresh sources of power and renewed strength for the kind of free enterprise and democratic teamwork to which we are accustomed and to which we have committed our destiny as a nation.

EIGHTY-EIGHT YEARS OF SYNTHETIC RUBBER

RALPH F. WOLF

Mr. Wolf (B.S., Catholic University, 1932) has recently joined the Research Department of the Columbia Chemical Division, Pittsburgh Plate Glass Company, Barberton, Ohio. He was formerly technical director of Harwick Standard Chemical Company, Akron, Ohio, and has served as chief of the Synthetic Rubber Allocation Section of W.P.B. Mr. Wolf is the author of India Rubber Man—the Story of Charles Goodyear.

FOURSCORE and eight years ago our chemical forefathers brought forth on this globe a new material conceived in the laboratory and dedicated to the proposition that it was the first synthetic rubber.

Most people are inclined to think of synthetic rubber as the child of this decade. It comes as quite a surprise, even to chemists, to learn that the first crude attempts to solve the rubber puzzle were made eighty-eight years ago. Eighty-eight years takes us back to 1860, a year before the War of the Rebellion started. 1860 was the year Charles Goodyear died. It was the year the vulcanized rubber industry came of age—twenty-one years old.

All of us ought to feel a little humble to know that the foundations of the present multimillion-dollar synthetic rubber industry were laid that long ago. It is a human trait to feel that everything worth knowing has been discovered or developed within the scope of one's own lifetime. Consequently, it is well to remember that nearly ninety years ago there were men who were doing sound thinking on the same problems that interest rubber chemists today, and doing it without benefit of any background of pioneer research and without the equipment and facilities that we accept as commonplace.

The first great name in the story of synthetic rubber is that of the Englishman Greville Williams. Others before Williams had subjected rubber to destructive distillation. As early as 1833, Barnard had obtained a volatile oil, which he patented as a rubber solvent under the name "caoutchoucine." In 1834 both Justus Liebig and John Dalton obtained volatile distillates from rubber, but evidently considered them of little importance. There were others—Dumas, Gregory, Himly, and A. Bouchardat, to name a few.

In 1860 Williams made the first systematic attempt to isolate and examine the products present in rubber. He repeatedly purified the evil-smelling distillate he first obtained until he had a pure, low-boiling fraction that he named "isoprene." Historically, this was of great importance because isoprene or one of its relatives has been the basis of every synthetic rubber ever made, the Thiokols excepted. Williams did not stop at tearing rubber down. From his isoprene he built up—something.

Writing in *Philosophical Transactions* in 1860, he said:

> If isoprene be left in a partly filled bottle for some months it gradually loses its fluidity and at last becomes quite viscid; at the same time it is found to have acquired powerful bleaching properties. It is, in fact, ozonized.
>
> On submitting ozonized isoprene to distillation, if the temperature be kept as low as is compatible with the volatilization of the unaltered portion of the oil, a colorless limpid fluid distills over, having the composition and boiling point of pure isoprene. As the operation proceeds, the fluid thickens, causing the temperature to rise somewhat suddenly. The ozone at this point instantly begins to act with energy, a cloudy vapor arises accompanied by an intensely sharp odour, and the contents of the retort instantly solidify to a pure white, spongy elastic mass, having, when successfully prepared, but slight tendency to adhere to the fingers. When pure it is opaque; but if allowed to become exposed to the air, especially when warm, it becomes transparent, first on the edges and subsequently throughout the whole mass. When burnt it exhales a peculiar odour hitherto considered to be characteristic of caoutchouc itself. . . .
>
> I am anxious to call attention to the fact that the atomic constitution of caoutchouc appears to bear some simple relation to the hydrocarbons resulting from its decomposition by heat. The composition of caoutchouc coincides with that of

isoprene, as found by analysis, to a degree which is remarkable when we consider that caoutchouc in addition to being non-crystalline is scarcely capable of purification by chemical means.

It should be emphasized that Williams himself did not claim that he had produced synthetic rubber. No less an authority than William Henry Perkin, however, considered that Williams had probably produced some sort of polymer of isoprene.

A Frenchman, Gustave Bouchardat, was next to point out the close connection between isoprene and rubber. In 1875 he carried out quantitative experiments on the decomposition of caoutchouc, obtaining something over half a pound of isoprene from eleven pounds of rubber. He heated this material in sealed tubes and noted that it became viscous and acquired elasticity. In 1879 he heated one part of isoprene with twelve parts of concentrated hydrochloric acid for twenty hours in a sealed tube. After distilling to remove the isoprene hydrochlorides, he obtained a product which he said had "the elasticity and other properties of rubber itself. It is insoluble in alcohol, it swells up in ether, also in carbon bisulphide in which it dissolves after the fashion of natural rubber." Bouchardat performed other chemical tests on it and decided that "all these properties seem to identify this polymer of isoprene with the source of isoprene, namely rubber."

In England, William Tilden confirmed Bouchardat's experiment in 1882 and worked out the chemical structure of isoprene. In a speech that year before the British Association for the Advancement of Science, he said:

> The other peculiarity of isoprene is its conversion into true india-rubber when brought into contact with certain chemical reagents such as strong aqueous hydrochloric acid as noticed by Bouchardat or nitrosyl chloride as observed by myself. It is this character which gives it a somewhat practical interest, for, if it were possible to obtain this hydrocarbon from some other and more accessible source the synthetical production of india-rubber could be accomplished.

Tilden himself was the first to find that other source. In 1884 he made isoprene by passing turpentine vapor through a red-hot tube. He was professor of chemistry at Mason College, Birmingham, at the time. He made a considerable quantity of isoprene, some of which he used; the rest he bottled up, set aside on his laboratory shelf, and apparently forgot about for eight years.

Meanwhile, in 1887, Otto Wallach reported in the German *Annalen:* "When isoprene is placed in sealed tubes and is exposed to the action of light for a long time, a product is formed, which, when treated with alcohol forms a tough mass, resembling rubber; this material became more or less hard on exposure to air."

Unaware of Wallach's results, Tilden on May 18, 1892, read a paper before the Birmingham Philosophical Society, in which he said:

> Specimens of isoprene were made from several terpenes in the course of my work on those compounds and some of them I have preserved. I was surprised a few weeks ago at finding the contents of the bottles containing isoprene from turpentine entirely changed in appearance. In place of a limpid, colourless liquid, the bottle contained a dense syrup in which was floating several large masses of a solid of a yellowish colour. Upon examination this turned out to be india-rubber. The change of isoprene by spontaneous polymerization has not, to my knowledge been observed before. . . . The artificial india-rubber like natural rubber appears to consist of two substances, one of which is more soluble in benzene or carbon bisulphide than the other. . . . The artificial rubber unites with sulphur in the same way as ordinary rubber forming a tough, elastic compound.

That Tilden really had some kind of synthetic rubber has never been questioned except by a few die-hard Germans bent on getting full credit for their country. The spontaneous change was confirmed by the great rubber chemist, C. O. Weber, in 1904 and by Samuel Pickles in 1910.

Isoprene monopolized all the attention in these early efforts at polymerization. Not until 1900 did the Russian Kondakov report that methyl isoprene could be converted into an elastic mass by heating with potash. The following year Thiele showed that piperylene also polymerized to a rubbery product when left in the dark for a long time. On first thought, it seems strange that the polymerization of butadiene was not reported for almost another decade. Butadiene had been discovered by Caventou in 1863. Probably because it is a gas at ordinary temperatures,

it was not found to undergo polymerization until 1910. In that year, Lebedev reported that on heating it yielded a rubberlike material.

The Tilden of 1882, who was optimistic about artificial rubber, lost his optimism with the passing years and shortly before 1900 he wrote to the *Kew Bulletin*:

> I have tried everything I can think of as likely to promote the change but without success. The polymerization proceeds very slowly, occupying according to my experience several years and all attempts to hurry it result in the production not of rubber but of colophene, a thick, sticky oil quite useless for the purposes to which rubber is applied.

A little later and a little more discouraged, he wrote in the same publication

> It is a question whether the process could be made commercially productive even if a suitable reagent could be found to effect the transformation because the yield of isoprene from turpentine is very small. . . . If isoprene were obtainable at a low cost from other sources it might be found possible to utilize the hydrochloric acid process, though I doubt it.

In 1907 Tilden said that synthetic rubber was commercially impossible at any price and advised those who had invested in plantations to go on putting their money in them because the future of the industry hung on the cultivation of the rubber tree.

Tilden's work marked the end of the first, or academic, phase of the search for synthetic rubber. Even while he was losing faith, the second phase was getting under way—chemists everywhere were devoting more and more time to the problem. It is not difficult to understand why this was so. In 1900 the average price of rubber in this country was 50.6 cents. Demand for rubber was increasing. Production of wild rubber was dropping off, owing mainly to overexploitation of trees and of native populations. Production from plantations was negligible; in fact, the idea had not even been proved to be a sound one. In 1906 rubber sold for $1.50 a pound; in 1907 and 1908, for $1.40; in 1909, for $2.20; and in 1910, the price hit $3.06. Obviously, the chemist who could have discovered how to make rubber in the laboratory at that time, before the plantations were well established, could have become one of the world's wealthiest men.

The quest for synthetic rubber developed into a bitter international race between Germany and England. Turpentine was abandoned early as a satisfactory raw material for the synthesis. It was not cheap, the supply was not dependable, and it was subject to market manipulation the same as rubber. Consequently, processes were developed for the production of isoprene and its relatives from carbide, coal tar, petroleum, and starch.

In Germany, the Bayer chemists, Fritz Hoffman and Carl Coutelle, in March 1909, produced isoprene from paracresol by a complicated process. This was done by catalytically hydrogenating to methyl cyclohexanol, oxidizing to β-methyl adipic acid, converting to the diamide and then to methyl tetramethylene diamine, and finally obtaining isoprene by exhaustive methylation.

Coutelle tried all sorts of likely and unlikely chemical and physical agents to hasten the thickening into rubber and, in August 1909, obtained a rubber by simply heating the isoprene in closed tubes for about eight days at a temperature below 200° C. The discovery received wide publicity and has been cited by German writers ever since as the first synthetic rubber.

In the following year, there occurred an example of the almost simultaneous discovery of a scientific fact by more than one observer. In England, Francis Matthews found that sodium caused isoprene to polymerize rapidly and applied for a patent on October 25. Three days later, Harries, in Germany, communicated the same discovery to Bayer. In resulting patent suits, Matthews' claims were upheld in both countries.

Fritz Hoffman started the manufacture of isoprene rubber on a small scale in Germany in 1910. The price of natural rubber, remember, was over $3.00 a pound. Isoprene at that time was difficult to make, and Hoffman turned to methyl isoprene, which was easier. Today the reverse is true. The manufacture of so-called methyl rubber began to assume some importance in Germany, but it was halted by the crash in natural rubber prices, which occurred as a result of plantation rubber finally starting to become a factor.

An interesting offshoot of the work on methyl rubber in Germany was the indepen-

dent discovery of organic accelerators of vulcanization in that country. Methyl rubber aged badly. Hoffman and his associates, Delbrück and Meisenburg, found that addition of materials such as piperidine gave a better product and also hastened vulcanization. They tried their discovery in natural rubber and arrived at the same results achieved by Oenslager and Spence in the United States some years earlier.

ENOUGH for the moment about Germany; let's see what was going on in England during this period. The English were far from idle. In 1908 E. H. Strange, of the firm of Strange and Graham, started his head chemist, Francis Matthews, on the search for artificial rubber. Matthews first proposed to follow the acetone-to-isoprene route, but shortly devised a better method, using amyl alcohol from fusel oil. The alcohol was treated with hydrogen chloride, and the chloride obtained by fractionation was converted by further chlorination into a mixture of dichloropentanes. This was passed over soda lime at 470° C. and gave a 40 percent yield of isoprene.

William Perkin, son of the great dye chemist, was working on the same subject and was approached by Strange, who proposed an alliance. The group was soon joined by Sir William Ramsay, Chaim Weizmann, and half a dozen other foremost English scientists. A mass attack was started on the problem of obtaining rubber from various alcohols. Fernbach, of the Insitut Pasteur in Paris, was enlisted to find a better source of amyl alcohol than fusel oil, which was relatively scarce. Fernbach decided that the reason fusel oil was scarce was that all previous fermentation work had been aimed at producing as little of it as possible. He deliberately sought to make fusel oil the principal product and after about eighteen months' work discovered how to make huge quantities of it by the proper fermentation of potato starch. From the fusel oil, both amyl and butyl alcohols were obtained, the former being used for isoprene and the latter for butadiene. Rubber was made from both of these by Matthews' sodium process.

The group was so enthusiastic about its prospects that it organized the Synthetic Products Company in 1912 to manufacture acetone, alcohols, and synthetic rubber. The output of butyl alcohol and acetone kept the company going for some time, but no serious attempt was ever made to produce rubber because the price of crude was down to 48 cents in 1913.

The smoldering rivalry between German and English chemists burst into open flames in 1912 and was an omen of the greater conflict that was to start two years later.

Perkin began it with a lecture he delivered in London in 1912. It is not hard to understand his antipathy for German chemists. He was the son of the man who discovered the first coal-tar dye. England, in his father's time, had little interest in chemistry. The government did not support the senior Perkin's plan for developing the industry. Universities were above educating chemists. The development of organic dyes slipped out of the hands of the English and into the hands of the Germans, who appreciated what they had and built it into the immensely valuable dye industry. By 1912 English textile makers were paying millions of dollars annually for German dyes.

In his lecture Perkin made it clear that he thought little of Hoffman and Coutelle's 1909 discovery. He said:

> As regards this synthesis, it seems to me desirable in view of statements which have been made abroad to review the work of earlier investigators and particularly that of Tilden in England and Bouchardat in France in order that I may emphasize the fact, as I particularly wish to do, that much of the credit of the pioneer work in this subject belongs to this country and France. It is surprising that Professor Harries should doubt whether Tilden really had synthetic rubber in his hands in 1882 and 1884 and should advance the view that certain samples produced in 1909 by Bayer and Company were the first true samples of synthetic rubber. . . . The specimens of crude synthetic rubber which Tilden undoubtedly prepared were made many years before Bayer and Company produced rubber by their process.

Perkin went even further back and credited Williams and Bouchardat with the discovery. Of the former, he said:

> It is true that this substance . . . was not at his disposal in sufficient quantity for him to definitely identify it with rubber, which it undoubtedly contained. [And of the latter he remarked] Bouch-

ardat in 1875 showed that isoprene is converted by heating in a sealed tube into a mixture which there can be no doubt contained a considerable portion of rubber.

Harries came back swinging in the *Annalen* about six months later. He wrote:

It is only about two years since my last paper on synthetic rubber was published and this paper has produced a remarkable literary activity. . . . This result would have been very welcome had there not appeared less agreeable features, which probably never before appeared to such an extent in any other field of chemistry. I refer to the introduction of nationalistic motives into scientific research, and to the broadcast discussion of questions of priority in scientific matters in the daily press, or in certain journals which usually are not selected for such discussions.

On July 15, 1912, W. H. Perkin, Junior, delivered a lecture before the London Section of the Society of Chemical Industry. . . . The purpose of this address is quite apparent, namely, the presentation of the discoveries in the field of rubber chemistry as purely English accomplishments. One can only shake his head as Perkin now seeks to interpret the old, meager statements of Williams relating to the auto-oxidation products of isoprene so that it will appear that Williams really had synthetic rubber in his hands. Perkin again returns to the experiments of Bouchardat and Tilden who claim to have polymerized isoprene to rubber by means of hydrochloric acid and he explains, contrary to my statements, that the observations of these investigators are undoubtedly correct. . . . If Mr. Perkin, Junior, is so much interested in the synthetic preparation of rubber by means of the action of hydrochloric acid on isoprene, I beg that he himself try at least once to rediscover the experimental conditions which were hidden by Bouchardat and Tilden. If then Perkin can so describe the reaction that it may be successfully repeated every time, I will admit without reservation the priority of Bouchardat and Tilden.

The German-English rivalry cropped out at the eighth International Congress of Applied Chemistry, which was held in New York in the fall of 1912. At this meeting Perkin announced the method of making synthetic rubber raw materials by fermentation processes and as a matter of interest displayed some samples of rubber made by Tilden years before. He was followed by the German, Carl Duisberg, who sneered at the English work, told of the discovery of synthetic rubber in Germany, and claimed all the credit for Hoffman. He said:

As far back as the '80's the Englishman, Tilden, claimed to have prepared artificial rubber from isoprene. . . . Neither Tilden nor his assistants, though they worked strenuously for years succeeded in repeating the experiments. . . . Dr. Fritz Hoffman is to be regarded as the real discoverer of synthetic rubber.

To emphasize his statements, Duisberg displayed a set of tires that had been made from Hoffman's synthetic rubber in Germany.

Nothing had been done in the United States about synthetic rubber during the fifty years preceding 1910. It was in this year that Brazilian rubber touched $3.06 a pound, and this unquestionably was responsible for the start of work in this country. Work on the problem was begun at about the same time by Richard Earle and L. P. Kyriakides at the Hood Rubber Company, in Watertown, Massachusetts, and by David Spence, at the Diamond Rubber Company, Akron, Ohio. The Americans actually contributed little that was original, their work following mainly along lines that had already been plotted by the English and Germans.

Kyriakides was one of the first to realize the great importance of purity in his raw materials.

It has been our experience [he wrote] that impure samples of isoprene never gave a good sample of rubber. It follows, therefore, that the hydrocarbon obtained by the older pyrogenic depolymerization methods of terpenes at ordinary pressures must be separated from the accompanying trimethyl ethylene before it is subjected to polymerization.

All subsequent work has served to confirm this observation. Kyriakides' isoprene was made by reacting ethyl magnesium chloride with chloroacetone and catalytically dehydrating the methyl-2-butyleneoxide-1, 2 which resulted. This pure isoprene was found to polymerize with sodium much more rapidly than had been reported by Harries. Butadiene was made from acetaldehyde by a process quite similar to that used in Germany, and was also polymerized with sodium.

The method chosen by Kyriakides and Earle in 1913 as being the most suitable for commercial development involved the preparation of dimethyl butadiene from pinacol derived from acetone, a synthesis first worked out in Germany. Plans were made for large-scale production of methyl rubber from this diolefin at an estimated cost of 80 cents a

pound. Development work was abandoned when costs of wild rubber dropped sharply in this year and it also became evident that production on plantations was practical.

In summarizing his work, Kyriakides said:

> The preparation of hydrocarbons directly from the hydroxy compounds is the simplest of all known methods. . . . I believe that I have indicated the lines along which work will be done in the future for the commercial preparation of the hydrocarbons in the synthesis of "rubber." This problem will eventually be solved, although it will require a good many years of earnest and patient study. The starting point in these syntheses will most probably be alcohol, which can be produced fairly cheaply.

A prophetic statement!

Spence's 1910 work was also on isoprene, which he made initially from rubber itself in order to have raw material on which to test various polymerization catalysts. After selecting acetic anhydride and pyridine as the most promising catalysts, he made considerable rubber in 1911 from isoprene prepared from dipentene and turpentine. In the same year he made polymers of several substituted diolefins. After an intensive study of the relative merits of isoprene and dimethyl butadiene as starting materials, it was decided at the Diamond Company that the latter offered most promise. Both sodium and acetic anhydride were considered for catalysts, but no final choice was made by the time the work was dropped late in 1912 as a result of falling rubber prices.

The start of the first world war in 1914 caught the Germans without any rubber. They probably had never thought of its importance because rubber had not played much of a part in any other war in history. This time it was needed for gas masks, for dirigibles, for U-boat batteries, for telephone wires, and most of all for the tires on which trucks and autos and motorcycles moved. Because England controlled the sea, wild rubber could not get to Germany from South America or Africa. England owned the far-eastern plantations that by 1914 were outproducing the wild trees. People in the United States who wanted to buy rubber had to promise that it would not be transshipped to Germany. These promises were not always kept in the years before this country entered the war, but the offenders were barred from further dealings in plantation rubber. Twice in 1916 a submarine made the trip across the Atlantic, a feat never before accomplished, and each time took 500 tons of rubber back to Germany. Attempts were made to smuggle rubber out of this country in barrels of rosin, in bales of cotton waste, and even in jars of pickles. The small amounts that got through did not begin to supply the German need. Before the war was very old, all usable auto and bicycle tires in Germany were taken by the army. Even these were not enough, and trials were made of tires made of wood, of wood covered with leather or canvas, and of steel springs.

At first no one thought of using Fritz Hoffman's synthetic rubber from methyl isoprene because prewar plant trials had convinced German production men of its inferiority. The German need soon became so great that tests were made of some material left in Hoffman's 1910 plant, which had been partially dismantled after crude rubber prices fell. This material was found good enough to make hard-rubber cases for the batteries that drove the submarines, and it was decided to attempt its large-scale manufacture.

To make more methyl rubber, the Germans needed acetone, but the available acetone was needed for smokeless powder. More acetone could have been made from wood, but there was no wood to spare. It could have been made from grain or potatoes by fermentation processes, but these were badly needed for food. It could have been made from acetate of lime, but this had always been imported. Consequently, the Germans turned to acetylene from electric-furnace carbide. From this they made acetaldehyde by means of a mercurous sulfate catalyst; oxidized this to acetic acid, heated with lime to obtain calcium acetate; and dry-distilled this to acetone. The acetone was treated with aluminum and caustic to give an aluminum salt of pinacol, and this was distilled under pressure to give methyl isoprene.

An enormous number of experiments were run on ways of changing methyl isoprene into rubber. Three methods were finally chosen. By allowing the material to stand

in tin drums for six to ten weeks at 30° C., H-rubber (H = *hart*) was produced. This was found very satisfactory for battery cases and hard-rubber electrical parts. By the spring of 1918 a ton a day was being produced for these purposes. By storing methyl isoprene in iron drums for three to six months at 70°C., W-rubber (W = *weich*) was made. This was used in soft-rubber goods but was far less satisfactory than the H-rubber was in hard rubber. Solid tires made of W-rubber were better than wood or steel, but that was about all that could be said for them. They were very inelastic and chipped badly in cold weather. In warmer weather, cars had to be jacked up when not running to keep the tires from developing flat spots. A third form, B-rubber, was made by allowing methyl isoprene to stand in contact with sodium wire in a carbon-dioxide atmosphere. This was suitable for insulated wire.

Near the end of the war, the Germans were making rubber at the rate of 150 tons a month. Altogether about 2,350 tons were made. When the war ended, two plants that would have boosted production to 8,000 tons a year were under construction.

THE end of the war also marked the end of the second phase of the synthetic rubber story. Interest in the subject disappeared completely until 1925, when restrictions on production sent the price of natural rubber to $1.25 a pound and a number of companies took up the question again. This was the start of what might be called the modern period in the history of synthetic rubber. In 1926 prices fell back to 35 cents, and everyone except Du Pont, I. G. Farben, and the Russians again lost interest. Du Pont's continued work led to the discovery of the chloroprene rubbers; I. G.'s work, to the bunas; the Russians concentrated on improving sodium-polymerized butadiene rubber.

Neoprene, which is the trade name given by Du Pont to its chloroprene rubbers, was discovered by a Catholic priest who was not looking for synthetic rubber at all, but was only interested in what he could do with acetylene gas. He was Julius A. Nieuwland, who came to this country from Belgium in 1880, when he was two years old. After graduating from Notre Dame, he entered Catholic University, in Washington, D. C., in 1903. There he became interested in chemistry and began the study of the reactions of acetylene that he pursued the rest of his life. In 1906 he happened to pass acetylene into a solution of copper chloride. The gas was absorbed as he expected, but at the same time he noticed a peculiar new odor he could not explain. He suspected formation of some new gas, but he was unable to isolate it. For fourteen years he returned to this experiment from time to time. In 1920 he hit upon the key. Instead of copper chloride, he was using the ammonium salt and the proper catalyst and succeeded in trapping not only a new gas but also a yellowish oil. After another year's work the new arrivals were identified. Acetylene itself consists of two atoms of hydrogen and two of carbon. The new gas had twice as many—two acetylene molecules had joined hands. In the case of the yellow liquid, three acetylene molecules had joined together. The gas was monovinyl acetylene; the liquid, divinyl acetylene.

Divinyl acetylene is extremely reactive, even more so than acetylene. Left to itself, it thickens into a jelly and then into a hard resin, which explodes with little encouragement. In spite of this, Nieuwland kept experimenting with the material and in 1923 found that when he treated it with sulfur chloride he obtained a soft, elastic solid. In 1925 he mentioned this fact at a chemical meeting in Rochester, and it immediately aroused the interest of Du Pont chemists who were present. They had been trying for several years to make synthetic rubber from acetylene without success. Arrangements for patent royalties were made, and the Du Pont chemists began working with Nieuwland. Their work with the explosive liquid was disappointing, because no two batches ever turned out alike. Finally, they began experimenting with the gas, monovinylacetylene, and Nieuwland suggested that it be reacted with hydrogen chloride. This was done, and a thin clear liquid, chloroprene, resulted. Chloroprene differs from isoprene chemically only in the fact that it contains one chlorine atom in each molecule. Practically, the difference between the two

materials is very great, chloroprene polymerizing about seven hundred times as fast as isoprene.

Actually, polychloroprene (Neoprene) should not be called synthetic rubber; it has chlorine in its molecules, and rubber has none. Identical or not, it was the first practical material ever made that would do all that rubber would do just as well or even better. Neoprene was put on the market in 1931 and sold for $1.05 a pound at a time when rubber cost five cents. But Neoprene resisted oil and grease and was far better than rubber for gasoline hose, engine mounts, printing rolls, and similar items, and its use continued to mount until a million pounds a month were being used just prior to the second world war when rubber was still freely available. During the war, production and consumption of Neoprene reached a total of 46,000 tons a year, but it has since fallen somewhat from this peak.

Brief mention might be made at this point of the other early American discovery in rubberlike materials. As in the case of Neoprene, this product was not at all like rubber chemically but did some jobs better. Again, as with Neoprene, its inventor was not looking for synthetic rubber. Thiokol, as this synthetic is called, was discovered by J. C. Patrick in 1928 when he was trying to develop new antifreeze mixtures for radiators. One day he mixed ethylene dichloride and sodium polysulfide and obtained a sticky, gummy mess that could not possibly be used in a radiator. Disgusted, Patrick threw the mess away. If he had had a one-track mind, he would have gone on looking for antifreeze and never have given his unsuccessful experiment another thought. Instead, he began to wonder if the gummy material might not be good for some other use. This proved to be the case. Thiokol is much more resistant than rubber or any of the other synthetics or aromatic solvents. It is not strong, however, and does not make very satisfactory articles by itself. When strength can be supplied by some other material, as by cotton fabric in hose, it is quite satisfactory, and there is a steady demand for about 1,000 tons a year.

During these years when American chemists were developing the first practical synthetic rubberlike materials, the Germans and Russians were also busy. Something else besides the high price of rubber in 1925 was spurring them on. Both were more concerned about national self-sufficiency than they were about the economics of synthetic rubber production and for that reason continued their work when other countries lost interest.

As early as 1915, the Russian chemist Ostromislenski had made butadiene by the catalytic action of aluminum oxide on a mixture of alcohol and acetaldehyde. Incidentally, this was the forerunner of the process used so successfully by the Carbide and Carbon Chemical Corporation in our own wartime butadiene program. In 1925 Lebedev modified Ostromislenski's method and developed it into a commercial process, the only direct method of converting ethanol into butadiene with a satisfactory yield. A mixture of catalysts that split off water and hydrogen at the same time was used. The butadiene so obtained was polymerized with sodium to the so-called SK rubber. Sodium polymerization presents certain difficulties, which caused the Germans to abandon this method. The principal trouble arises from local overheating, which is difficult to avoid in a mass-polymerization method. Unlike the Germans, the Russians stayed with the sodium process and solved the problems involved. This was accomplished mainly by changing from a batch process employing sodium rods to a continous process in which butadiene in the vapor phase is polymerized by a sodium dispersion. Fairly large-scale production of SK rubber is reported to have begun in 1932, and by 1939 capacity was reported at 90,000 tons annually by our own Department of Commerce.

About 1925 the Germans had begun to concentrate on butadiene rather than on isoprene (methyl butadiene) or methyl isoprene (dimethyl butadiene) because it is structurally the simplest molecule that will polymerize readily. The butadiene was made from electric-furnace acetylene.

Using sodium, they first developed a series of synthetic rubbers called the numbered bunas (Buna 85, Buna 115, etc.). The first two letters of the name stood for butadiene,

the second two for natrium (sodium), and the numbers indicated the molecular weights in thousands. The numbered bunas were never very satisfactory except for hard rubber. Consequently, the Germans decided to abandon sodium polymerization and concentrate their efforts on emulsion polymerization, since this appeared more practical.

The idea of emulsion polymerization was not new, the first patent for this method having been taken out by Bayer in 1912 for the polymerization of isoprene in an aqueous solution of egg albumen, starch, or gelatine. A similar patent in 1913 covered similar treatment of butadiene. Quite possibly, the first conception of this process resulted from analogy with the latex of the rubber tree.

Polymerization times that at first were measured in weeks were gradually reduced to days and then to hours by development of improved catalysts, but quality of the polybutadiene still left a great deal to be desired.

In their efforts to improve their butadiene rubber, the Germans had wondered if they might be able to overcome its leathery character by modifying it with some other material; in other words, by making a copolymer instead of a single polymer. In the late twenties, at the same time that they were trying to improve their straight polybutadiene rubber and were actually getting into pilot-plant production on it, they were trying all sorts of materials to make softer and more flexible bunas. The two best materials that were found were styrene (vinyl benzene) and acrylonitrile (vinyl cyanide). The inventors of the styrene copolymer, which came to be known as Buna S, were Eduard Tschunkur and Walter Bock, of the Leverkusen works of I. G. Farben. The acrylonitrile copolymer, which was called Buna N, was invented by Tschunkur and Erich Konrad. Buna S was a general-purpose type of synthetic rubber, and Buna N was for oil-resistant uses.

Development of the two butadiene copolymers led Dr. Schacht, of the Hitler cabinet, to announce to the world at large in 1934 that the synthetic rubber problem had been solved and that production would start in a short time. A year later production amounted to 25 tons a month. The timetable called for production of 200 tons a month in 1936 and 1,000 tons a month by 1938. This schedule was not met, but production did reach about 5,000 tons a year in 1938, and is reported to have reached 60,000–70,000 tons annually in 1941. German rubber-goods manufacturers were compelled to use every pound produced whether they wanted to or not in order to reduce costly imports, which upset Germany's foreign trade balance.

First word about the straight butadiene polymers, the numbered bunas, reached this country in the late twenties after both Neoprene and Thiokol had been discovered here. In 1930 Standard Oil of New Jersey and I. G. Farben formed the Joint American Study Company for the commercial testing and licensing of new processes developed by either party for making chemical products from petroleum raw materials. One of the buna processes was the first thing taken up by the new company. In 1933 an agreement was made with General Tire Company for testing the polybutadiene rubber, and substantial samples were sent to the American factory. The report on this material was quite unfavorable.

The copolymer of butadiene with acrylonitrile (Buna N) was introduced in this country in 1937. An explosion at the Du Pont Neoprene plant left rubber-goods manufacturers here without a domestic source of oil-resistant rubber, which by this time had become one of their regular raw materials, albeit in small volume. A small shipment of I. G.'s Perbunan (Buna N) was made, and it worked so well that a small but steady demand was created.

In addition to the Buna N, samples of Buna S began to reach this country in 1937, and American rubber men acquired some familiarity with it. During the middle thirties, some of the leading American rubber chemists, namely Dinsmore, Sebrell, and Semon, visited the I. G. plants in Germany to observe what was being done in the emulsion polymerization of butadiene copolymers. By the end of the decade, two of the four largest American rubber companies were producing butadiene copolymers on a pilot-plant scale, and in 1940 the other two obtained licenses from Standard Oil to make Buna S and Buna N. Standard, as an equal partner in JASCO

(Joint American Study Company) had the rights to the buna rubbers outside Germany.

In 1940 our government began to be concerned about rubber supplies and showed some interest in making synthetic rubber. Chemists of the four largest companies recommended Buna S as the rubber that should be made. This was an obvious choice since Buna S was the only general-purpose synthetic with which the American rubber-goods manufacturing industry had any background of experience. At first, plans were drawn for four 2,500-ton units. During the summer of 1941, the size of these plants was increased to 10,000 tons apiece. When we were attacked in December 1941, they were jumped to 30,000 tons each. As the war progressed and more and more of the natural rubber lands were lost, additional plants were contracted for until a total designed capacity of 705,000 tons was reached.

For several reasons, a detailed account will not be given here of this country's wartime Buna S program. The story has already been told adequately several times (e.g., SM, January, 1944). Further, it would be out of place here because, actually, no fundamental scientific discovery was made. Without question, the construction of our wartime synthetic rubber industry was a tremendous chemical engineering feat, perhaps the greatest the world has ever seen. The fact remains, however, that we duplicated an existing product, we did not discover a new one. The nationalistic-minded claim that GR-S should not be called Buna S because it is far superior. This is quite true, but basically the two materials are the same—approximately three parts of butadiene to one of styrene copolymerized in emulsion. The modifiers, additives, and improved techniques discovered in this country do not alter the fact that GR-S and Buna S are fundamentally identical and that Buna S was entirely a German discovery.

Not so with butyl rubber. Like Neoprene and Thiokol, butyl is purely an American discovery, although it is an outgrowth of an earlier German invention. About 1932 I. G. chemists learned how to polymerize a refinery gas, isobutylene, into a white, rubberlike solid by using a boron fluoride catalyst at low temperature. Polyisobutylene, or "Oppanol," as it was called by the Germans, is a saturated material; that is, the carbon atoms have no free bonds, and, consequently, it is a completely unreactive material. Unlike rubber, it will not vulcanize or combine with sulfur. As a result, the German discovery was of no more practical value than was natural rubber before Charles Goodyear learned how to vulcanize it in 1839. Polyisobutylene becomes sticky when warmed. Articles made of it will not hold their form, but gradually flow into a shapeless mass.

This flexible, rubberlike solid attracted the interest of two young Standard Oil chemists, William J. Sparks and Robert M. Thomas, in 1937. They reasoned that if some other material could be made to copolymerize along with the isobutylene, the product might have a few carbon atoms with reactive double bonds. This proved to be very clear thinking. Sparks and Thomas found that, when they added 2 or 3 percent of butadiene, isoprene, or methyl isoprene to the isobutylene and polymerized the two together, they had a useful synthetic rubber. There was just enough unsaturation in a molecule of butyl rubber to permit it to vulcanize. It then lost all the faults of the original German Oppanol and would hold its shape as well as natural rubber. On the other hand, it retained all the inertness of the original polymer and was but little attacked by oxygen or strong chemicals.

Butyl rubber is made by a unique method of polymerization. Isobutylene and isoprene are first cooled to 150° below zero. At this point they are brought into contact with the aluminum chloride catalyst, and the butyl rubber forms instantly. The particles of rubber are quenched in hot water to stop the activity of the catalyst, and the product is then dried and massed.

Butyl was not introduced to the rubber-goods manufacturing industry until 1940. Consequently, rubber chemists knew little about it when they had to decide what kind of synthetic the government was going to make; they picked Buna S because they were familiar with it and knew that the Germans were using it. First plans did not call for

the government to make any butyl at all. Standard Oil started to build a small plant of its own. After the war started the government took over this plant and increased its capacity to 7,200 tons a year. When it developed that Buna S was unsatisfactory for inner tubes and that butyl was superior even to natural rubber, the facilities for producing butyl in this country were increased to 68,000 tons. The reason for butyl's marked superiority over natural rubber in this one application is that it holds air ten times longer.

Eighty-eight years have only brought synthetic rubber to the threshold of maturity. Although hundreds of thousands of tons of it have been used in the past five years, most of it was used because of dire necessity. General-purpose synthetic rubber has not yet really had to compete on merit. Today it faces the hottest kind of competition, as natural rubber is permitted to enter this country freely. This competition will undoubtedly stimulate the development of better and cheaper synthetic rubbers. Eventually, the natural-rubber plantations may go the way of the indigo fields of India and the madder farms of France, and *Hevea brasiliensis* may join those other ex-aristocrats of the vegetable kingdom, the camphor and cinchona trees, in the limbo to which they have been relegated by the organic chemist.

PARABLE OF THE TWO YOUNG PROSPECTORS AND THEIR RICH UNCLE

TWO Young Prospectors, their eyes shining with a Vision of Discovery, went to their rich Uncle and said: "Uncle, we wish to search for precious ores in the Unknown Mountains. Will you grubstake us?"

Now the rich Uncle was worldly-wise and cautious. He replied: "Perhaps. But first you must tell me what precious ores you propose to find, in what condition and quantity, the length of time that it will take you to accomplish this, and the amount of money that it will cost. Here is a neatly printed Form with appropriate blanks for this information."

"But Uncle," exclaimed the Prospectors, "how can we truthfully tell you this? No one has explored the Unknown Mountains. We may search for years and find nothing. If we could foretell what we would find, it would not be prospecting."

"Young men," replied the Uncle, "I have Responsibilities to my Family; I cannot risk their money on what may be a fool's errand. Let me grubstake you to work in the Old Diggings. You may not discover precious ores, but you can be sure of finding enough of the baser metal to guarantee the safety of my money."

The First Young Prospector took the neatly printed Form, filled it out, and set off for the Old Diggings. The years passed, each year bringing its small but regular return of the baser metal. At last the Uncle called in the First Prospector. "You have been so faithful in the Old Diggings," he said, "that now I will grubstake you to prospect in the Unknown Mountains." But the First Prospector was no longer young; no longer were his eyes shining with the Vision of Discovery. He now supervised a crew of young diggers. "Why should I leave my comfortable home, my assured income from the baser metal?" he thought. He did not regret that he no longer had the Vision. He had forgotten it.

The Second Young Prospector rejected his Uncle's proposal to work in the Old Diggings. He became a Teacher. Of his limited salary he put aside what little he could spare from his frugal living, until the sum became a little grubstake. With this, in vacation time and on a small scale, he prospected the Unknown Mountains. He did not find the great mother lode of precious ore; prospectors from other lands did that; but he did discover its outcropping, and other men, following the trail that he had blazed, gradually transformed the Unknown Mountains into the New Diggings.

The years passed, and the Second Prospector was poor and no longer young, but his eyes still shone with the Vision of Discovery, and now and then a spark of that Vision kindled a flame in the eyes of one of the young prospectors who sat at the Teacher's feet, and now and then one of these young prospectors, grubstaking himself as best he could, set out to prospect in other Unknown Mountains.

K. STARR CHESTER

PRINCIPLES OF REALLY SOUND THINKING

MAX BLACK

Dr. Black, a native of Baku, Russia, was educated in European universities and received his Ph.D. from the University of London. He came to this country for the first time in 1938. In 1940 he was made professor of philosophy at the University of Illinois and since 1946 has held the same post at Cornell. Dr. Black is editor of the Journal of Symbolic Logic *and author of the very successful* Critical Thinking (*Prentice-Hall, 1946*).

THERE is thinking and thinking; we shall be concerned with the second kind. Textbooks are still full of impracticable principles of logic for which men of affairs have unqualified contempt. It is time to be more realistic. But for the dead hand of Aristotle and the lingering influence of medieval scholasticism, logicians would not have the impudence to refer to practical thinking as "invalid" or "poor." Such emotive language should be avoided. The principle of identity should have taught academic logicians that Really Sound Thinking can be nothing if not Really Sound.

We are interested in the way the man in the street should think. Formal logic should be left to mathematicians and scientists who get paid for trafficking in abstractions. Symbolic logic should be left to those lonely thinkers who understand the symbols.

There are two major principles of really sound thinking:

A. Think only as a last resource.

B. Trust your feelings.

THINK ONLY AS A LAST RESOURCE

The really sound thinker knows thinking to be an uncomfortable, disturbing, and antisocial occupation. Consider the attitude of Rodin's statue "The Thinker." This is not the favorite posture of a successful executive or a regular guy.

Modern life fortunately provides a number of defenses against the early onslaught of thinking. The radio is always close at hand —use it. The company of others, preferably of the opposite sex, is to be strongly recommended. If the irritation is too severe, one may retreat to bed until restored to a healthier frame of mind.

It has to be confessed, however, that complete protection against thinking still remains to be achieved:

> But men at whiles are sober
> And think by fits and starts
> And if they think, they fasten
> Their hands upon their hearts.
>
> —A. E. HOUSMAN

Conscientious adherence to the next principle will go far to palliate the discomforts of unavoidable and involuntary thinking.

TRUST YOUR FEELINGS

The logic texts have created the fiction of Logical Man, coldly calculating the probabilities of alternative hypotheses, willfully blind to human sentiment and passion. Do you want to be this kind of philosophical monster, interminably vacillating between conflicting conclusions? Of course not. In any matter of serious concern, you will *feel* strongly that a certain conclusion *must* be right. This is the clue to success in really sound thinking. Let yourself go—think in technicolor.

Suppose you are worried about the possibility of war with Russia. You will notice in yourself a tendency to think of Stalin as a bloodthirsty ruffian, dripping with the gore of murdered innocents. Dwell upon the notion —let your blood pressure rise. In a short time you will *feel* strongly enough to be able to stop *thinking* altogether. In really sound thinking, it is the conclusion that counts, not the premises. Trusting your feelings will quickly provide you with satisfying, heartwarming conclusions.

THE two principles of really sound thinking can be illustrated by the following

maxims, widely accepted by successful practitioners.

1. *If you must stick to the point, be sure it's blunt.* The natural human reaction to contact with a sharp point is violent motion in reverse. Such animal wisdom is deeply significant. It's the dead butterfly that stays on the point. Cultivate judicious irrelevance.

Example: Does John Smith deserve a raise in salary?
Blunted point: Doesn't everybody deserve a raise?
Really sound reasoning: Of course they do! Who is John Smith to be favored at the expense of everybody else?

2. *What's in it for me?* Remember that a really sound thinker is practical. And what can be more practical than concern for one's own interests? The chief advantage of this maxim is the strong light it throws upon the truth of many a debatable proposition.

Example: Should educational facilities be improved in the South?
Really sound reasoning: What's in it for me? Nothing—*I* don't live in the South.
Conclusion: NO. (Notice the directness and incisiveness of the method.)

3. *It all depends on who says it.* Men are easier to classify than arguments—attend to the man, not the argument. (For classifying the speaker, see principle B above.)

Example: Should Congress be reorganized?
Really sound reasoning: Who says so? X? Oh—he ran for Congress three times unsuccessfully.
Conclusion: You can't trust *him.*

4. *A million people can't be wrong.* It would clearly be undemocratic, not to say snobbish, to think otherwise. We can't all be Gallups, but we have a ready fund of popular wisdom to hand in the form of proverbs. Make frequent use of such axioms as "Human nature never changes," and "An ounce of experience is worth a peck of talk," and, especially, "It will all be the same in a thousand years." The last is particularly consoling.

Example: Can we prevent another war?
R.s.r.: I've *seen* men fighting. You'll never change human nature. After all, it will all be the same in a thousand years.

This method can be usefully supplemented by the use of identical propositions, such as "East is East, West is West," "Business is Business," "A man's a man for a' that." These are best introduced by the words "after all." Even a logician can hardly dispute the truth of such tautologies.

5. *The exception proves the rule.* Corollary: The more exceptions, the better the rule. This popular maxim hardly needs recommendation. It has the great advantage of allowing us to make simple generalizations in an intolerably complex world. (See also Maxim 8 below.)

Example: You say women are no good at physics. What about Madame Curie?
R.s.r.: The exception proves the rule! (Absolutely conclusive, as r.s.r. should be.)

6. *It's all right in theory but it won't work in practice.* We might almost say: *Because* it's right in theory, it won't work in practice. This maxim is very useful in puncturing the pretentions of experts.

Example: Should we support the United Nations?
R.s.r.: (You know what!)

7. *Consistency is the hobgoblin of little minds.* None of the great thinkers from Socrates to Korzybski have been consistent. Who are you to improve upon their practice? The sciences are notoriously full of unresolved contradictions. If scientists don't care, why should you?

Example: You say that we ought to work for universal free trade, but insist on raising American tariffs.
R.s.r.: I contradict myself? Very well, I contradict myself.

8. *Truth is always pure and simple.* Notice the purity and simplicity of this maxim. Oscar Wilde denied its truth, and see what happened to him (compare Maxim 3). Anything too complicated for translation into Basic English is unworthy the attention of a really sound thinker. The truth must be incapable of shocking the Johnston (formerly Hays) Office. In any case, truth is too precious to be lightly squandered. It is better to hold it a closely guarded hostage far back in reserve.*

Example: Is there anything in psychoanalysis?
R.s.r.: Of course not. Why, I can't even understand it. And it isn't fit to print.

*With acknowledgments to Peter Fleming.

9. *Take care of the sound and the sense will take care of itself.* This is perhaps the most important of all the maxims of really sound thinking. "There is a great advantage in names" (Mark van Doren). Be sure you get the greatest benefit out of the names you use.

If you trust your feelings as you should (principle B above), you should have little trouble in finding the right name. Thus, the *Management Review* lately recommended the use of "Income Account" instead of "Profit and Loss Account," "Earnings" instead of "Profits," "Reinvested in the Business" instead of "Added to Surplus." You get the idea?

Example: On being questioned about the implications of a legislative program no really sound thinker would say, "I don't know." This is better: "It is totally unreasonable to expect a blueprint which answers every question which can arise day after tomorrow in this distraught earth, when no man knows for twenty minutes at a time what is going to happen" (Senator Vandenberg, quoted in the *New York Times,* April 18, 1947).

10. *Never argue with a man who is wrong.* Corollary for married ladies: Never argue with a husband. For the purposes of this maxim, a man who is wrong is easily identified as one who (a) is an unsound thinker, (b) refuses to see that you are right, or (c) has an unwholesome look (see Maxim 3 above).

The careful reader will have noticed that the reasoning used in the above exposition of the principles of really sound thinking provides numerous further illustrations of the principles discussed.

ARCTIC TERN

Bird of snow,
white, white, whiter than anything
(flake on feathered flake,
wing spread to wing)
over what leagueless ocean
have you come,
past what green isles,
to rest at last
(unwearied, graceful still)
upon a floating skyscraper of ice—
or to a lonely sandbank
where the sun
leans shadowless?
Now, the long flight over
(flight of wind,
flight of drifting snow)
find once again your nest,
your mate . . .
and in the hangar of the Arctic night
rehearse your Odyssey of flight.

MAE WINKLER GOODMAN

MAN'S CONTROL OVER CIVILIZATION: AN ANTHROPOCENTRIC ILLUSION

LESLIE. A. WHITE

After taking his Ph.D. at the University of Chicago in 1927, Professor White taught at the University of Buffalo and served at the same time as a curator of the Buffalo Museum of Science. He remained in Buffalo until 1930, when he joined the staff of the University of Michigan, where he is now professor of anthropology. His specialty is the evolution of culture.

. . . numerous survivals of the anthropocentric bias still remain and here [in sociology], as elsewhere, they bar the way to science. It displeases man to renounce the unlimited power over the social order he has so long attributed to himself; and on the other hand, it seems to him that, if collective forces really exist, he is necessarily obliged to submit to them without being able to modify them. This makes him inclined to deny their existence. In vain have repeated experiences taught him that this omnipotence, the illusion of which he complacently entertains, has always been a cause of weakness in him; that his power over things really began only when he recognized that they have a nature of their own, and resigned himself to learning this nature from them. Rejected by all other sciences, this deplorable prejudice stubbornly maintains itself in sociology. Nothing is more urgent than to liberate our science from it, and this is the principal purpose of our efforts.—EMILE DURKHEIM.

. . . it appears like a grandiose dream to think of controlling according to the will of man the course of social evolution.—WM. F. OGBURN.

THE belief that man controls his civilization is widespread and deeply rooted. Customs and institutions, tools and machines, science, art, and philosophy are but man's creations and are therefore here only to do his bidding. It lies within man's power, therefore, to chart his course as he pleases, to mold civilization to his desires and needs. At least so he fondly believes.

Thus we find a distinguished British scientist, the late Sir James Jeans, assuring us that

> We no longer believe that human destiny is a plaything for spirits, good and evil, or for the machinations of the Devil. There is nothing to prevent our making the earth a paradise again—except ourselves. The scientific age has dawned, and we recognize that man himself is the master of his fate, the captain of his soul. He controls the course of his ship, and so, of course, is free to navigate it into fair waters or foul, or even to run it on the rocks.

Mr. Stanley Field, president of the Chicago Natural History Museum, appeals to anthropologists in espousing free will:

> But if we listen to the anthropologists, who can scientifically demonstrate that it is not color of skin, or type of hair or features, or difference of religion, that creates problems between peoples, but factors for which man is responsible and which he can control or change if he will, then we shall at least come within sight of that better world which we now realize we must achieve if we are not finally to perish as victims of our own perversity.

Professor Lewis G. Westgate (SM, August 1943, 165) tells us that man can "take the problem of his future in hand and solve it. . . . The mind that can weigh the infinitely distant stars . . . track down the minute carriers of disease . . . dig the Panama Canal . . . can solve its social problems when and if it decides to do so."

It would seem that the salvation of an earlier era has become the social reconstruction of today: we can achieve it if we will; if we fail, it is because of our "perversity."

When, however, we look for examples of man's control over culture, we begin first to wonder, then to doubt. We shall not begin our inquiry by asking if two world wars in one generation are evidence of planning or perversity, or whether Germany and Japan were crushed and Soviet Russia made dominant in Eurasia in accordance with a far-sighted plan or as a result of blindness and folly. We shall start with something much more modest. During the past century we have witnessed attempts to control tiny and relatively insignificant segments of our culture, such as spelling, the calendar, and the system of weights and measures, to name but a few. There have been repeated and heroic attempts to simplify spelling and make it

more rational, to devise a more rational calendar, and to adopt an ordered system of weights and measures instead of the cumbersome, illogical agglomeration of folk measurements we now use. But what successes can we point to? Reform in spelling has been negligible. We have succeeded to a considerable extent but not wholly in eliminating the *u* from such words as *honor*. But to do away with silent letters, such as the *b* in *lamb*, is too big a mountain for us to move. And such spellings and pronunciations as *rough, cough, dough,* and *through* are much too strong to yield to our puny efforts. It usually takes a great political and social upheaval to effect a significant change in spelling or a calendrical system as the French and Bolshevik revolutions have made clear. And as for the metric system, it has found a place among the little band of esoterics in science, but yards, ounces, rods, pints, and furlongs still serve—awkwardly and inefficiently—the layman.

We begin to wonder. If we are not able to perform such tiny and insignificant feats as eliminating the *b* from *lamb*, or modifying our calendar, how can we hope to construct a new social order on a world-wide scale?

Let us look about us further. Men and women are forever contending with fashions. Man perennially rebels against his attire. It is often uncomfortable, injurious to the health at times, and, some men think, the ordinary costume is unaesthetic, the formal attire ridiculous. But what can he do? He must wear his coat and tie no matter how hot the weather. He is not permitted to wear pink or blue shoes. And as for "evening clothes" —he must submit to them or stay home. Man's vaunted control over civilization is not particularly conspicuous in this sector.

But if man is helpless, woman is an abject slave, in the grip of fashion. She must submit to any change, no matter how fantastic or ugly. To be sure, she may not realize that the new designs are fantastic and ugly at the time; "the latest style" can becloud a woman's judgment. But one has only to browse through an album of old snapshots to realize that beauty, grace, and charm do not dominate the course of fashion.

And as for women's skirts! First they are short; then they are long. A distinguished anthropologist, Professor A. L. Kroeber, of the University of California, has made a very interesting and revealing study of the dimensions of women's dresses over a considerable period of time. He found that "the basic dimensions of modern European feminine dress alternate with fair regularity between maxima and minima which in most cases average about fifty years apart so that the full-wave length of their periodicity is around a century." The rhythms are regular and uniform. Women have nothing to say about it. Even the designers and creators must conform to the curve of change. We find no control by man—or woman—here, only an inexorable and impersonal trend. When a maximum point on the curve is reached, the trend is reversed and skirts lengthen or shorten as the case may be. Women are helpless; they can do nothing but follow the trend. When the curve ascends they must shorten their dresses; when it descends, they must lengthen them. It may seem remarkable that a great class of citizens who cannot even control the dimensions of their own skirts will nevertheless organize themselves into clubs to administer the affairs of the world. We shall return to this point later.

Few men would undertake to repair an automobile or a radio without some understanding of its mechanism. We tend more and more nowadays to leave medicine and surgery to those who know how. Knowledge and skill are required even to make good pies or home brew. But in matters of society and culture everyone feels qualified to analyze, diagnose, and prescribe. It is one of the premises of democracy that not only do the people rule, but they have the requisite knowledge and understanding to do it effectively. In matters political, one man's view is as good as another's.

When, however, we examine the knowledge and understanding with which the affairs of the nation are administered, we begin again to wonder. We find the most august authorities espousing different and even contradictory views on such subjects as inflation, the function of labor leaders, the divorce rate, the popularity of crooners, and so on. This is a picture of the anarchy of ignorance.

When we turn from matters of national proportions, such as the cause of inflation, to lesser problems, we are not always reassured. Does capital punishment diminish the number of murders? Does the use of alcohol affect the divorce rate? Why do people keep dogs? They are noisy, dirty, unhealthful, useless, and expensive. To say that they are kept because people like them is merely to phrase the problem in a different form. Why don't they "like" raccoons? They are cute, cleanly in their habits, and very amiable.

The fact is we don't really know very much about the civilization we live in. Let us take one of the simplest and most elementary questions imaginable: Why does our society prohibit polygamy? Other societies permit plural mates, and Western Europe once did also. But now we feel very strongly about it. We will put a man in a prison for years if he takes unto himself more than one wife at one time. His wives may be perfectly satisfied with the arrangement and he may have injured no one. Yet we put him in gaol. Why?* Why not have one more wife and one less schoolmarm? There are, to be sure, ready answers to these questions: polygamy is "wrong," "immoral," "undemocratic," etc. But practices are not prohibited because they are "wrong"; they are wrong because they have been prohibited. It is not wrong to buy and sell whisky now; it was while the Eighteenth Amendment was on the books. And as for democracy, we permit a man to have two yachts if he can afford them, why not two wives?

I know of no really adequate answer to this question in such literature of social science as I am acquainted with. As a matter of fact, the question is very seldom raised. I have looked for it in a great number of treatises on sociology and anthropology written during the past quarter century without finding it. Some social scientists of the latter half of the nineteenth century tried to explain the prohibition of polygamy, but we cannot accept their conclusions.

The fact is we are ignorant. We do not know the solution to such an elementary problem as singular or plural mates. And in our day, we have not reached the point of asking such questions, to say nothing of answering them. As Archibald MacLeish has said, "We know all the answers, but we have not yet asked the questions." Over half a century ago the great French savant, Emile Durkheim, commented upon the immaturity of social science as follows:

> In the present state of the science we really do not even know what are the principal social institutions, such as the state, or the family; what is the right of property or contract. . . . We are almost completely ignorant of the factors on which they depend . . . ; we are scarcely beginning to shed even a glimmer of light on some of these points. Yet one has only to glance through the works on sociology to see how rare is the appreciation of this ignorance and these difficulties.

Despite the progress that has been made since *The Rules of Sociological Method* was written, this statement has a certain relevance today. If the science of society and civilization is still so immature as to be unable to solve such a tiny and elementary problem as the prohibition of polygamy, where are the knowledge and understanding requisite to planning a new social system, to constructing a new world order? One would not expect a savage craftsman, whose best tools are made of chipped flint, to design and build a locomotive.

Let us have a look at this civilization man thinks he controls. The first thing we notice is its antiquity. There is no part of it, whether it be technology, institutions, science, or philosophy, that does not have its roots in the remote past. The lens of the new 200-inch telescope, for example, is made of glass. Glass emerged from the manufacture of faïence in ancient Egypt, which in turn originated apparently as a by-product of burning bricks and pottery, which followed the use of sun-dried brick and, earlier, mud daubs of Neolithic, or even Paleolithic, huts. The United Nations can be traced back to primitive tribal councils and beyond. Modern

* We recall a recent instance in which a man was sent to the penitentiary for marrying some twelve women without ever bothering with the ritual of divorce. Had he been less honorable or chivalrous and lived with each woman without the formality of marriage, his "crime" would have been much less. This man served society well in a municipal railway system. His numerous wives pressed no complaint. Why did society feel it necessary to incarcerate him?

mathematics goes back to counting on one's fingers, and so on. Culture is as old as man himself. It had its beginnings a million-odd years ago when man first started to use articulate speech, and it has continued and developed to the present day. Culture is a continuous, cumulative, and progressive affair.

Everyone—every individual, every generation, every group—has, since the very earliest period of human history, been born into a culture, a civilization, of some sort. It might be simple, crude, and meager, or it might be highly developed. But all cultures, whatever their respective degrees of development, have technologies (tools, machines), social systems (customs, institutions), beliefs (lore, philosophy, science), and forms of art. This means that when a baby is born into a cultural milieu, he will be influenced by it. As a matter of fact, his culture will determine how he will think, feel, and act. It will determine what language he will speak, what clothes if any he will wear, what gods he will believe in, and how he will marry, select and prepare his foods, treat the sick, and dispose of the dead. What else *could* one do but react to the culture that surrounds him from birth to death? No people makes its own culture; it inherits it ready-made from its ancestors or borrows it from its neighbors.

It is easy enough for man to believe that he has made his culture, each generation contributing its share, and that it is he who controls and directs its course through the ages. Does he not chip the arrowheads and stone axes, build carts and dynamos, coin money and spend it, elect presidents and depose kings, compose symphonies and carve statues, worship gods and wage war? But one cannot always rely upon the obvious. It was once obvious that the earth remained stationary while the sun moved; anyone could see that for himself. We are now approaching a point in modern thought where we are beginning to suspect that it is not man who controls culture but the other way around. The feat of Copernicus in dispelling the geocentric illusion more than four hundred years ago is being duplicated in our day by the culturologist who is dissipating the illusion that man controls his culture.

Although it is man who chips arrowheads, composes symphonies, etc., we cannot explain culture merely by saying that "man produced it." There is not a single question that we would want to ask about culture that can be answered by saying, "Man did thus and so." We want to know why culture developed as it did; why it assumed a great variety of forms while preserving at the same time a certain uniformity; why the rate of cultural change has accelerated. We want to know why some cultures have money and slaves and others do not; why some have trial by jury, others ordeal by magic; why some have kings, others chiefs or presidents; why some use milk, others loathe it; why some permit, others prohibit, polygamy. To explain all these things by saying, "Man wanted them that way," is of course absurd. A device that explains everything explains nothing.

Before we go very far, we discover that we must disregard man entirely in our efforts to explain cultural growth and cultural differences—in short, culture or civilization as a whole. Man may be regarded as a constant so far as cultural change is concerned. Man is one species, and, despite differences of skin, eye, and hair color, shape of head, lips, and nose, stature, etc., which after all are superficial, he is highly uniform in such fundamental features as brain, bone, muscle, glands, and sense organs. And he has undergone no appreciable evolutionary change during the past 50,000 years at least. We may, therefore, regard man as a constant, both with regard to the races extant today and with regard to his ancestors during the past tens of thousands of years. Man has a certain structure and certain functions; he has certain desires and needs. These are related to culture, of course, but only in a *general,* not a specific, way. We may say that culture as a whole serves the needs of man as a species. But this does not and cannot help us at all when we try to account for the variations of specific cultures. You cannot explain variables by appeal to a constant. You cannot explain the vast range of cultural variation by invoking man, a biological constant. In England in A.D. 1500 there was one type of culture; in Japan, another. Neither culture can be explained in terms of the

physical type associated with it. Culture underwent change in England between A.D. 1500 and 1900, as it did in Japan. But these changes cannot be explained by pointing to the inhabitants in each case; they did not change. Plainly, we cannot explain cultures in terms of Man.

Nor can cultural differences be explained in terms of environment. Quite apart from the difficulty of accounting for differences in musical styles, forms of writing, codes of etiquette, rules of marriage, mortuary rites, etc., in terms of environment, we soon discover that even economic, industrial, and social systems cannot be so explained. The environment of central Europe so far as climate, flora, fauna, topography, and mineral resources are concerned has remained constant for centuries. The culture of the region, however, has varied enormously. Here again we see the fallacy of explaining the variable by appeal to a constant.

If, then, we cannot explain cultures in terms of race or physical type, or in terms of psychological processes, and if appeal to environment is equally futile, how *are* they to be accounted for and made intelligible to us?

There seems to be only one answer left, and that is fairly plain—after one becomes used to it, at least. Culture must be explained in terms of culture. As we have already noted, culture is a continuum. Each trait or organization of traits, each stage of development, grows out of an earlier cultural situation. The steam engine can be traced back to the origins of metallurgy and fire. International cartels have grown out of all the processes of exchange and distribution since the Old Stone Age and before. Our science, philosophy, religion, and art have developed out of earlier forms. Culture is a vast stream of tools, utensils, customs, and beliefs that are constantly interacting with one another, creating new combinations and syntheses. New elements are added constantly to the stream; obsolete traits drop out. The culture of today is but the cross section of this stream at the present moment, the resultant of the age-old process of interaction, selection, rejection, and accumulation that has preceded us. And the culture of tomorrow will be but the culture of today plus one more day's growth. The numerical coefficient of today's culture may be said to be 365,000,000 (i.e., a million years of days); that of tomorrow, 365,000,000 + 1. The culture of the present was determined by the past, and the culture of the future will be but a continuation of the trend of the present. Thus, in a very real sense, *culture makes itself*. At least, if one wishes to explain culture scientifically, he must proceed *as if* culture made itself, *as if* man had nothing to do with the determination of its course or content. Man must *be* there, of course, to make the existence of the culture process possible. But the nature and behavior of the process itself is self-determined. It rests upon its own principles; it is governed by its own laws.

Thus, culture makes man what he is and at the same time makes itself. An Eskimo, Bantu, Tibetan, Swede, or American is what he is, he thinks, feels, and acts as he does because his culture influences—"stimulates"—him in such a way as to evoke these responses. The Eskimo or American has had no part in producing the culture into which he was thrust at birth. It was already there; he could not escape it; he could do nothing but react to it, and that on its own terms. The English language, the Christian religion, our political institutions, our mills, mines, factories, railroads, telephones, armies, navies, race tracks, dance halls, and all the other thousands of things that comprise our civilization are here in existence today. They have weight, mass, and momentum. They cannot be made to disappear by waving a wand, nor can their structure and behavior be altered by an act of will. We must come to terms with them as we find them today. And they will be tomorrow what their trend of development in the past dictates. We can only trot along with them, hoping to keep up.

MAN has long cherished the illusion of omnipotence. It is flattering and comforting to his ego. In days gone by, man has believed that he could control the weather; countless primitive peoples have had rituals for making rain, stilling high winds, or averting storms. Many have had ceremonies by means of which the course of the sun

in the heavens could be "controlled." With the advance of science, however, man's faith in his omnipotence has diminished. But he still believes that he can control his civilization.

The philosophy of science—of cause-and-effect relationships, of determinism—has been firmly established in the study of physical phenomena. It is well entrenched in the biological field also. Psychology may have demonstrated the operation of the principle of cause and effect, of determinism, in mental processes, and may have dispelled the notion of free will for the *individual*. But social science is still so immature as to permit one to find refuge in a collective free will. As Professor A. L. Kroeber has recently observed:

> I suspect that the resistance [to the thesis of cultural determinism] goes back to the common and deeply implanted assumption that our wills are free. As this assumption has had to yield ground elsewhere, it has taken refuge in the collective, social, and historical sphere. Since the chemists, physiologists, and psychologists have unlimbered their artillery, the personal freedom of the will is thankless terrain to maintain. Culture they have not yet attacked; so that becomes a refuge. Whatever the degree to which we have ceased to assert being free agents as individuals, in the social realm we can still claim to shape our destinies. The theologian is piping pretty small, but the social reformer very loud. We are renouncing the kingdom of heaven, but going to establish a near-millennium on earth. Our personal wills may be determined, but by collectivizing them we can still have social freedom.

Primitive man could believe that he could control the weather only because he was ignorant; he knew virtually nothing of meteorology. And today it is only our profound and comprehensive ignorance of the nature of culture that makes it possible for us to believe that we direct and control it. As man's knowledge and understanding grew in meteorology, his illusion of power and control was dissipated. And as our understanding of culture increases, our illusion of control will languish and disappear. As Durkheim once observed, "As far as social facts are concerned, we still have the mentality of primitives."

Needless to say, this is not the view taken by many today who look to science for our salvation. Far from expecting belief in our ability to control to diminish with the advance of social science, many people expect just the reverse. It has become the fashion these days to declare that if only our social sciences were as advanced as the physical sciences, then we could control our culture as we now control the physical forces of nature. The following quotation from a letter published in *Science* (**103**, 25–26) is a conservative statement of this point of view:

> For if, by employing the scientific method, men can come to understand and control the atom, there is reasonable likelihood that they can in the same way learn to understand and control human group behavior. . . . It is quite within reasonable probability that social science can provide these techniques [i.e., for "keeping the peace"] if it is given anything like the amount of support afforded to physical science in developing the atomic bomb.

The premise underlying this view is unsound. It assumes that wars are caused, or at least made possible, by ignorance and the lack of social control that goes with ignorance. It assumes that, given understanding through generous grants of funds to social scientists, wars could be prevented—the "peace could be kept." The lack of understanding and realism displayed here is pathetic. The instinct of self-preservation of a society that subsidized atom-bomb inventors rather than social scientists holding views such as these is a sure one. Wars are not caused by ignorance, nor can "the peace be kept" by the findings of social scientists. Wars are struggles between social organisms —called *nations*—for survival; struggles for the possession and use of the resources of the earth; for fertile fields; coal, oil, and iron deposits; for uranium mines; for seaports and waterways; for markets and trade routes; for military bases. No amount of understanding will alter or remove the basis of this struggle, any more than an understanding of the ocean's tides will diminish or terminate their flow.

But the fallacy of assuming that we can increase and perfect our control over civilization through social science is even more egregious than we have indicated. To call upon science, the essence of which is acceptance of the principles of cause and effect and determinism, to support a philosophy of free will is fairly close to the height of absurdity. Verily, Science has become the modern magic! The belief that man could

work his will upon nature and man alike *if only he had the right formulas* once flourished in primitive society as magic. It is still with us today, but we now call it science.

No amount of development of the social sciences would increase or perfect man's control over civilization by one iota. In the man-culture system, man is the dependent, culture the independent, variable. What man thinks, feels, and does is determined by his culture. And culture behaves in accordance with its own laws. A mature development of social science would only make this fact clear.

The philosophy of free will and omnipotence is rampant in the field of education. "Educators," high-school principals, commencement orators, and others never seem to tire of telling the world that its salvation lies in education. Naturally, one does not expect meat packers to preach vegetarianism, and it is reasonable for educators to advocate schooling and to urge the support of schools and colleges. But one could wish that they would place their propaganda on a somewhat higher intellectual plane; after all, one expects more of educators than of vendors of soap and cigarettes. The fact is, however, that the appeals and claims of educators are seldom more realistic than soap and cigarette ads. Education will no more cure the ills of society than cigarette X will soothe the nerves, or soap Y beautify the hands, of the user. As a matter of fact, the premise upon which the educator's claim rests is, if anything, less sound than the premise of the advertisements, for these are at least relevant, whether sound or unsound, whereas the educator's premise is hardly even relevant. The soap or cigarette will certainly have *some* effect upon the user. But education does not, strictly speaking, have an effect upon culture at all; *it is a part of it*. Education, whether formalized in the classroom or informal in the course of everyday living, *is one of the processes of culture:* it is the means of transmitting culture from one individual, one group, or one generation, to another. Being a process *within* the system that is culture, it is therefore fallacious to think of it as acting upon culture from the outside. It is not people who control their culture through education; it is rather the other way around: *education is what culture is doing to people,* namely, equipping them with ideas, beliefs, ideals, codes of conduct, tools, utensils, machines, etc.—in short, determining how they shall think, feel, and behave.

The position taken here will of course be vigorously denied and opposed. People do not give up their illusions easily. As the distinguished anthropologist, A. L. Kroeber, has put it:

> Our minds instinctively resist the first shock of the recognition of a thing [cultural determinism] so intimately woven into us and yet so far above and so uncontrollable by our wills. We feel driven to deny its reality, to deny even the validity of dealing with it as an entity; just as men at large have long and bitterly resented admitting the existence of purely automatic forces and system in the realm that underlies and carries and makes possible the existence of our personalities: the realm of nature.

A common reaction—verbal reflex—to the theory of cultural determinism is to brand it "fatalistic" or "defeatist." The choice of these words is significant. Why is it that, when one employs the principle of cause and effect in the realm of physical and chemical phenomena, no one cries "fatalism," but the instant one applies it to human cultural phenomena this accusation leaps forth? Why is it that an admission of our inability to control the weather brings forth no charge of "defeatism," whereas this reproach is promptly leveled against anyone who recognizes man's inability to control the course of civilization?

The reason is fairly plain. "Fatalism" implies free will; "defeatism," omnipotence. When atoms, cells, or tissues behave in accordance with their nature and properties, no one calls it fatalistic because no one expects freedom of choice and action of them. But when one asserts that cultural phenomena have a nature of their own and consequently must behave in terms of their nature, the response is not an acceptance of the principle of cause and effect but a charge of "fatalism."

> To many educated minds [the great English anthropologist, E. B. Tylor, wrote many years ago], there seems something presumptuous and repulsive in the view that the history of mankind is part and parcel of the history of nature, that our thoughts, wills, and actions accord with laws as definite as those which govern the motion of waves, the combi-

nation of acids and bases, and the growth of plants. . . . If law is anywhere it is everywhere.

We have combined "a scientific realism, based on mechanism," says Alfred North Whitehead, with "an unwavering belief in the world of men and of the higher animals as being composed of *self-determining organisms*" (emphasis ours). He feels that this "radical inconsistency" is responsible for "much that is half-hearted and wavering in our civilization. It . . . enfeebles . . . [thought] by reason of the inconsistency lurking in the background."

Implicit in the charge of "fatalism" and "defeatism" is the further notion of refutation. To brand a view "fatalistic" is, to many minds, to call it false as well. "Cultural determinism is fatalistic and therefore false," is about the way the reasoning would go if it were made explicit. "How can determinism exist?" is the question that is implied but unspoken. "Determinism is unthinkable." And so it is to one possessed by a philosophy of free will.† We find this point of view rather well expressed by Lawrence K. Frank in a recent article, "What is Social Order?" (*Amer. J. Soc.*, 49, 475):

> Perhaps the major obstacle we face today, therefore, is this essentially defeatist tradition expressed in the various conceptions of social order described earlier, as above and beyond all human control. . . . In this situation, therefore, we can and we must find the courage to view social order as that which must be achieved by man himself.

Of course man can "find the courage" to view social order as something "that must be achieved by himself." It does not take courage to do this, however; what is required is ignorance and hope. "Must find the courage," "must be achieved by man himself," is hardly the language of science. It is, rather, exhortation and rhetoric—of a type with which we have long been familiar: "if we will but purpose in our hearts. . . ."

No doubt the first to question man's control over the weather, the first to claim that the winds will blow, the rain and snow fall, the seasons come and go, in accordance with their own nature rather than in obedience to man's wish and will expressed in spell and ritual, were accused of "fatalism" and "defeatism," if, indeed, they were not dealt with more harshly. But, in time, we have come to accept our impotence in this regard and to become reconciled to it. If it be argued that man cannot control the weather because that is a part of the external world, whereas culture, being man-made, is subject to his control, it must be pointed out that the exact opposite is the case. It is precisely in the realm of the external world that man's control is possible. He can harness the energies of rivers, fuels, and atoms because he, as one of the forces of nature, lies *outside* their respective systems and can therefore act upon them. But man, as an animal organism, as a species, lies *within* the man-culture system, and there he is the dependent, not the independent, variable; his behavior is merely the function of his culture, not its determinant. Both theoretically and practically, therefore, it is possible for man to exert more control over the weather than over culture, for he can exert *some* control over the former even now (dispel fog on airfields) and he may increase this control in the future.‡ But he exerts no control whatever over his culture, and theoretically there is no possibility of his ever doing so.

THE usual reactions to this manifesto of cultural determinism are as unwarranted as are the assumptions of free will, from which, of course, these responses flow. After expostulating on the theme of "fatalism" and "defeatism," the conventional protest goes on to demand: "What is the use then of our efforts? Why should we try to do anything

† Note that we have said "possessed by," rather than "believes in." Philosophies possess, hold, animate, guide, and direct the articulate, protoplasmic mechanisms that are men. Whether a man—an average man, typical of his group—"believes in" Christ or Buddha, Genesis or Geology, Determinism or Free Will, is not a matter of his own choosing. His philosophy is merely the response of his neuro-sensory-muscular-glandular system to the streams of cultural stimuli impinging upon him from the outside. What is called "philosophizing" is merely the interaction of these cultural elements within his organism. His "choice" of philosophic beliefs is merely a neurological expression of the superior strength of some of these extrasomatic cultural forces.

‡ Since the above was written the press has carried reports of experiments in "rain making," in which dry ice is dropped into clouds from an airplane.

to improve our lot if we have no control over our culture? Why not just sit back and let the evolutionary process take care of everything? Of what use could a science of culture possibly be to us if control lies beyond our grasp? What good is an understanding of culture if there is nothing we can do about it?"

These questions are naive and betray a lack of understanding of what the cultural determinist—the culturologist—is trying to say. The determinist does not assert that man is irrelevant to the culture process. He knows full well that the contrary is the case; that man is an absolute prerequisite to it; that without man there could be no culture. He realizes very clearly the essential role that man plays in the system that is man-and-culture. What the culturologist contends is that in this system the human organism is not the determinant; that the behavior of the culture process cannot be explained in terms of this organism but only in terms of the culture itself; that the growth and changes among the Indo-European languages, for example, cannot be accounted for in terms of man's nerves, muscles, senses, or organs of speech; or in terms of his hopes, needs, fears, or imagination. Language must be explained in terms of language.

But to turn to some of the specific questions with which dissatisfaction with the philosophy of determinism is expressed: In the first place, we cannot "just sit back" and let the evolutionary process take care of all our problems. While we live we are confronted by our culture and we must come to terms with it. Even just sitting back, incubating a case of dementia praecox, is "doing something about it." So is committing suicide; as a matter of fact, suicide rates for various societies provide excellent indexes of cultural determinism. In some societies, the rate is high; in others, suicide is virtually nonexistent. This is not because suicide determinants are more abundant in the chromosomes of some populations than of others. It is due to the fact that the cultural determinants vary: hara-kiri is something that a culture does to an organism that, of its own nature, tends to persevere in that form of motion we call "life." It is obvious that we cannot avoid reacting to our culture.

To assume that the process of cultural evolution will take care of everything without effort on our part is of course absurd, and constitutes no part of the determinist's philosophy. Of course we must exert ourselves while we live, we cannot do otherwise. But the question is not "Who does the work, ourselves or cultural evolution?" It is obvious that the energy is expended by or through human beings. The question is, *What determines the nature, the form and content of this expression of energy in the culture process, the human organism or the extrasomatic culture?* The answer is of course fairly obvious—after a small amount of reflection. Let us consider two groups of human organisms, A and B. Group A raises taro, catches fish, carves wood, makes no pottery, speaks a Polynesian language, has chiefs but no currency, is nonliterate, drinks kava, is greatly concerned with genealogy, and so on. Group B mines coal and iron, speaks Welsh, imports its food from the outside, uses money, is literate, drinks ale, etc. Now the question is, Why does each group behave as it does? Is it that one group of organisms possesses traits or characteristics—genes, instincts, or psychological tendencies—that cause them to drink kava rather than ale? This is, of course, ridiculous; the one group of organisms is fundamentally like the other biologically. It is obvious that each group of organisms behaves as it does because each is reacting to a particular set of cultural stimuli. It is obvious also that a consideration of the human organism is totally irrelevant to the question. Why is one group stimulated by one set of stimuli rather than by another? This is a cultural historical question, not a biological or psychological one. So, one is not so silly as to say, "Why should we mine coal or catch fish? Let our culture do it." The question is not Who mines the coal? but What is the determinant of this behavior? And, the culturologist points out the obvious: the culture is the determinant.§

§ A distinguished sociologist has trouble telling cart from horse in the man-culture equation: Professor Robert S. Lynd comes up with the verdict that culture does not "enamel its fingernails, or vote . . . but people do" (*Knowledge for What?* P. 27).

The reaction of many sincere, altruistic and conscientious people, upon being told that it is not they who control their culture and direct its course, is "Why then should we try to do good, to better our lot and that of mankind?" We have answered this question in part already. In the first place one cannot avoid trying to do something. As long as one accepts life and is willing to continue with it, he must exert himself. "Trying" is merely the name we give to the effort exerted in the process of living. To strive for this or that, therefore, is inseparable from our lives. But *what* one strives for and *how* his effort is expressed is determined by his culture. For example, the goal of one people may be eternal life in heaven, for which their terrestrial existence is but a preparation. The goal of another might be the good life "here below." One group may deny the reality of sickness; another may admit its existence and try to combat it. One group may use charms and incantations; another, clinics and laboratories. Whatever the goal and whatever the means employed to reach it are matters determined by the culture of the group.

But, it should be pointed out with emphasis, this is not a philosophy of defeatism or hopelessness by any means. Least of all does it declare that one's efforts do not count. The fact that one's efforts to stamp out tuberculosis are culturally determined in no way minimizes the effort or the result. A life saved is a life saved. A letter written to a congressman has an *effect,* too, no matter what kind or how much. A resolution on world affairs passed by a woman's club has a real function in society, although it may be a very different one from that imagined by the good ladies. The question we raise is not one of the value of effort or whether effort has consequences. Human effort is just as real as anything in the realm of the geologist. And effort is followed by consequences, just as effect follows cause in physics or geology. Living human beings cannot help but exert themselves, and everything they do counts for something in one way or another. Far from wishing to deny or ignore this, we wish to emphasize it. But this is not the question raised by the culturologist, the cultural determinist. What he claims is not that it is futile to try because what one does counts for nought, but that what one does, how he does it, and the end and purpose for which it is done is culturally determined, is determined by the culture of the group rather than by the free will of the individual or of the group. More than that, what a person or group desires is determined or at least defined by the culture, not by them. What constitutes the "good life" for any people is always culturally defined.

From the cultural determinist's point of view, human beings are merely the instruments through which cultures express themselves. A physician, saving lives each day, is an instrument through which certain cultural forces express themselves; if they were not there or if they were different, the organism in question would not be practicing medicine, or he would practice it in a different way. The gangster, evangelist, revolutionist, reformer, policeman, impoverished beggar, wealthy parasite, teacher, soldier, and shaman are likewise instruments of cultural action and expression; each is a type of primate organism grasped and wielded by a certain set of culture traits. It is only the inveterate habit of thinking anthropocentrically that makes this point of view seem strange or ridiculous.

But, granting that what we do counts even though it is culturally determined, of what use is it to develop a science of culture if we cannot control civilization or direct its course? We have a science of pathology in order to combat disease, sciences of physics and chemistry to control the external world. But if we do not control our culture and cannot ever hope to control it, of what use would a science of culture be? We might begin our reply to this question by asking, Of what value is it to know the temperature of a star a million light-years away? Questions such as these betray a limited understanding of science. Science is not primarily a matter of control in the sense of harnessing rivers with hydroelectric plants or constructing uranium piles. Science is a means of adjustment; control is but one aspect of adjustment. Man finds himself in a universe to which he must adjust if he is to continue to live in it.

Mythology and science are means of adjustment; they are interpretations of the world in terms of which man behaves. There is, of course, a vast difference in terms of adjustment between a philosophy that interprets stars as a flock of snowbirds lost in the sky and one that measures their masses, distances, dimensions, and temperatures. This difference is a very practical one, too, in terms of the contribution that each philosophy makes to the security of life.

Our ancestors once thought they could control the weather, as contemporary savages still do. They finally outgrew this illusion, even going so far as to outgrow calling the new view "fatalistic" and "defeatist." But we do not think a knowledge and an understanding of weather and climate useless. On the contrary, we are devoting more time and money to meteorology now than ever before. Here again we see the situation in terms of adjustment rather than *control.* We may not be able to control the weather, but adjust to it we must. And knowledge and understanding make for more effective and satisfying adjustments. It would be advantageous if we *could* control the weather. But if we cannot, then weather prediction is the next best thing. And for prediction we must have knowledge and understanding.

So it is with culture. We cannot control its course, but we can learn to predict it. As a matter of fact, we make predictions all the time, and many of them are quite accurate: wheat production, traffic fatalities, freight carloadings, births, exhaustion of oil reserves, and many other matters are already within the reach of limited but nevertheless valuable prediction. If our ability to predict were greatly increased by the development and maturation of a science of culture, the possibilities of a rational, effective, and humane adjustment between man and culture and between one cultural segment and another would be increased accordingly. If, for example, a science of culture could demonstrate that the trend of social evolution is toward larger political groupings, then the chances of making the futile attempt to restore or maintain the independence of small nations would be lessened. If the trend of cultural evolution is away from private property and free enterprise, why strive to perpetuate them? If it could be shown that international wars will continue as long as independent, sovereign nations exist, then certain delusions now popular would find less nourishment and support. The fact is that culture has been evolving as an unconscious, blind, bloody, brutal, tropismatic process so far. It has not yet reached the point where intelligence, self-consciousness, and understanding are very conspicuous. Our ignorance is still deep-rooted and widespread. We do not understand even some of the most elementary things—the prohibition of polygamy, for example. In short, we are so ignorant that we can still believe that it is we who make our culture and control its course.

This ignorance is not surprising, however. It has not been very long since we gave up burning witches, cudgeling hysterics to drive out demons, and other savage practices. Even in technology, which tends to outstrip the social and ideological sectors, we have surpassed the savage at two points—fire making and the use of the bow and arrow—only within the past century or two. Chemical matches are but a little more than a century old, and within the past hundred years the bow and arrow was used in bison hunting on the American plains in preference to the best firearms available at the time. It was only yesterday, culturologically speaking, that a small portion of mankind began to emerge from a condition of savagery. For most of his career thus far man has subsisted wholly upon wild foods; less than 2 percent of human history, as a matter of fact, has elapsed since the origin of agriculture. Other significant indexes: some 0.7 percent of culture history since the beginning of metallurgy, 0.35 percent since the first alphabet, 0.033 percent since Galileo, 0.009 percent since the publication of Darwin's *The Origin of Species,* and only 0.002 percent since William Jennings Bryan and the Scopes trial. A mature, urbane, and rational civilization is not to be achieved in a mere million years from the anthropoid level.

It should be made clear that, if an adequate understanding should come about as a consequence of a science of culture, it would not have been "us" who achieved it but our

culture. In the interaction of elements in the culture process, those traits less effective in providing adequate adjustment in terms of understanding and control are gradually relinquished and replaced by more effective traits. Thus, bronze axes replace stone axes, ikons and spells give way to laboratories and clinics, and, finally, a science of human culture begins to challenge the primitive philosophy of omnipotence and free will. The new science will of course have to prove its superiority over the older view, just as astronomy, chemistry, and medicine have in other sectors of experience. The success of science—the philosophy of materialism, of cause and effect, of determinism—in the physical and biological sectors of experience encourages us greatly in the belief that this point of view and these techniques of interpretation will prove effective in the social sphere also.

Our role in this process is a modest one. Neither as groups nor as individuals do we have a choice of roles or of fates. Swedes are born into their culture just as Zulus, Tibetans, and Yankees are born into theirs. And each individual is thrust by birth into some particular place in the "magnetic field" of his culture, there to be molded by the particular organization of cultural influences that play upon him. Thus, he may have the belief that typhoid exists only in the mind, or is caused by witches or bacilli, thrust upon him—or "into his mind." He may be endowed with a belief in personal immortality, the efficacy of prayer, or the periodic law of Mendelyeev. He may be inspired to preach the only true faith to the heathen in distant lands, or to wear out his life in a genetics laboratory, or to believe that "only saps work." To be sure, the response of the human organism to cultural stimulation will vary with the quality of the organism. Some will be silk purses; others, sow's ears. The order in which an organism undergoes experiences is important too; the influence of events *a, b, c,* will not be the same as *a, c, b.* An experience will have one effect at fifteen; quite another at fifty. There is room, therefore, for almost infinite variety of permutation and combination in the experience of individual organisms.

Man discovers his place in the cosmos slowly and accepts it with extreme reluctance. Time was when his solid earth was planted in the center, the sun and stars spread upon the vault of heaven, and men and gods together acted out the drama of life and death. It was all so compact, so familiar, so secure. Then it was that man, like God, could cry, "Let there be light," and there was light. Like God, too, man was "omnipotent," if, however, to a lesser degree. With his magic formulas, his spells, prayers, charms, and rituals, mighty man could control the weather, the seasons, and even enlist the gods in the service of man. Now it is different. Man finds himself but one of innumerable animal species crawling about on an insignificant planetary speck, fighting, feeding, breeding, dying. Once the child of God, he now finds himself an ex-ape. But he has acquired a new faculty, one unknown among all other species: articulate speech. As a consequence of this, a new way of life has been developed: culture. But this culture, this mass of extrasomatic tools, institutions, and philosophies, has a life and laws of its own. Man is just beginning to understand this.

Man is wholly at the mercy of external forces, astronomic and geologic. As a matter of fact, it is rather disconcerting to think of how narrow is the margin within which man lives. Change the temperature, velocity, rainfall, or atmosphere of the earth but a little and life would cease. It is a curious and, from a cosmic viewpoint, momentary, concatenation of circumstances that has made life possible. Man did long rebel against his dependence upon these outside forces; to be wholly at their mercy was unendurable. As a matter of fact, man has employed his precious and unique gift of speech more to deny the facts of his existence than to improve upon them. But a certain portion of the human race has come at last to accept our dependence upon nature and to try to make the most of it.

And so it is with culture. Belief in our omnipotence has, as Durkheim says, always been a source of weakness to us. But we are now discovering the true nature of culture and we can in time reconcile ourselves to this extrasomatic order as we have to

the astronomic, geologic, and meteorologic orders. To give up magic and mythology, which promised much but yielded nothing—nothing but the soothing comfort of illusion—was a painful experience. But to receive and accept a science and a technology that promises less but achieves a great deal is to reach a goal most men are loathe to lose. We may believe that knowledge and understanding of culture will make for a more satisfactory life, just as these traits have been of value in physics and biology. To be sure, understanding culture will not, as we have argued here, alter its course or change the "fate" that it has in store for us, any more than understanding the weather or the tides will change them. But as long as man remains an inquiring primate he will crave understanding. And a growing Science of Culture will provide him with it.

WELL OF DESOLATION

There is a deep abyss of darkest night
Where man is lost and utterly forsaken,
When groping for a truth beyond his sight,
Or forging blindly toward a hill untaken.
A vast and cruel loneliness of spirit,
For which no hope or comfort can be found;
A well of bitter desolation—near it
The echoes of derisive laughter sound.

Copernicus and Galileo knew it,
Before they swung the earth around the sun;
Beethoven brought his mighty anguish to it,
And Shakespeare paced its black oblivion.
No thinnest ray of light can enter there,
Yet all things great are born of its despair.

KATHARINE O'BRIEN

CULTURAL DISCONTINUITY AND THE SHADOW OF THE PAST

JULES HENRY

Professor Henry has recently left the Bureau of Labor Statistics, U. S. Department of Labor, to join the Department of Sociology and Anthropology at Washington University. Shortly after the end of World War II he went to Japan as chief of the Analysis Section, Morale Division, U. S. Strategic Bombing Survey. Dr. Henry helps to edit the American Journal of Orthopsychiatry.

. . . in many respects there is no question of riding roughshod over national habits of life which have a deep symbolic meaning. The question is irrelevant because old ways are gone.[1]

THE persistence of some old, even of some ancient, institutions and attitudes is a fact of scientific experience. This persistence is often used to explain and even to justify the present; and it casts a shadow dark and long over the world of politics and international relations. The thesis of this paper is that in our culture there is an exaggerated tendency to explain present social behavior in terms of past institutions. It may be true in some cases that attitudes developed in connection with specific institutions may continue when the institutions have vanished, but I hope to show that the notion of persistence is frequently overworked and often lacks support in the realities of the social process. Hence, it becomes a block to sociological insight and should be supplanted by a useful hypothesis.

From the standpoint of cultural continuity, there are two possible ways of viewing social forms: One, the present method of the social sciences, is to view each form as evolving out of a preceding form or complex of forms and always retaining its connection with the past. This may be called the hypothesis of continuous historical development and transformation. Another way of viewing social forms is to consider them as capable of drastic, and sometimes even swift, transformation, of such character that the tie with the past is largely lost. This may be called the hypothesis of discontinuous social change. While at first blush the idea of discontinuity may seem antihistorical, I think that it can be shown that as a practical heuristic device, at least, the hypothesis has some value. It is useful, I believe, especially where changes of great magnitude occur. Even in a period of rapid and far-reaching change, of course, the new forms are imposed upon, or developed out of, the old. But in such situations the strength of the new forms may become so great as to generate a new system of social institutions and attitudes. Then, as change succeeds change in accordance with the internal laws of the new social form, the old ones give way before it until an entirely new social situation, having little or even no relevant connection with the past, has supplanted the old. The scientist will then do well to seek the explanation of contemporary events first in the nature of the new form; and only when he has canvassed the immediate sociological terrain will he make an excursion into the past. For the continuity has been broken. However the new forms may be traceable to the past, the change has overlaid, or absorbed and digested, the old culture. Such a change occurred in Europe, Great Britain, and the United States with the industrial revolution, and in Japan with the introduction of industrial capitalism. Within the present generation we may yet witness another one with the development of atomic energy.

The process of seeking the explanation of contemporary forms first in contemporary circumstances seems to me to be just good scientific method. On the other hand, there is a powerful psychological undertow in our culture toward the ocean of the past. It was, for example, a problem on the United States Strategic Bombing Survey in interpreting Japanese behavior. For there was a constant, and in my opinion, an exaggerated

tendency among us of the social science personnel to see in the behavior of the contemporary Japanese "survivals" or "remnants" of the "feudal pattern."

Spurious continuity. Although it seems that as old institutions disappear the attitudes associated with them tend to vanish gradually also, situations may arise in which there is an apparent congruence between a contemporary idea, rooted in a new situation, and an ancient one, based upon an ancient situation. In such a case there may be a tendency to refer the contemporary idea to the old situation, even though the contemporary idea may be based upon an entirely new set of circumstances. Thus, Lapiere says: ". . . in America . . . the old feudal class system has persisted only in the most fragmentary form. Of those fragments the most important is the feudal idea of the divine right of the aristocracy to rule . . . which . . . now appears as the biological interpretation of class differences."[2] Here Lapiere, while acknowledging that most of the old feudal-system-related patterns of behavior and attitude have fallen away, still thinks that in some way the "idea of the divine right of the aristocracy to rule" has persisted in our thinking. However, even were we to admit, merely for argument's sake, that Americans interpret class differences in biological terms, it is still possible to assume that this contemporary notion is derived not from feudal times but from modern theories of biological inheritance, and that what has occurred is not the wedding of biological notions with feudal "survivals," but the marriage of a poorly understood biological theory with our modern class structure. If we wished, then, to pursue this matter further, the problem would be to discover how this union of the modern American class system and the biological theories came about.

In this case, the American class system and an alleged belief in the "biological inheritance of class differences" appear to coincide with "feudal" attitudes. But there is a certain cultural support for this attitude in the new situation; and this support is precisely in our class system. However, the class system is contemporary and bears no relation to the ancient one.

In this connection the following from one of America's unsung early historians is pertinent:

> In America there is but little pride taken in genealogy, and it is a rare occurrence to meet a family who can trace their ancestral lineage farther back than two or three generations. Business is the great absorbing interest of all classes of society, and keeps them intent upon the present and the future; what is past cannot materially affect their business, and the indulgence of family reminiscences would only occupy their mind to the exclusion of other thoughts more available perhaps in a financial view. This is the substance of their reasoning, and hence the ignorance displayed by most families in ancestral knowledge.[3]

Odum and Moore's remarks on this score are also significant: ". . . in the Middle States frontier men and women were expected to stand on their own achievements rather than those of their ancestors."[4] And a final touch is given to this picture of the development of social differentiation in early America by Ernest Groves:

> It would be misinterpretation to insist that the frontiersmen maintained social equality. It is rather that they refused to accept artificial or vicarious distinctions . . . the basis for determining character traits rested upon personal behavior. The methods for giving or denying prestige were distinctly frontier measurements. The Westerners were willing to follow leadership, but only as it was supported by qualities they approved and admired.[5]

Thus, these data, and many others, suggest that whatever biological theories of class developed in America, they had no connection with Old World feudalism.

This peculiar circumstance in which a contemporary system resembles an old one, yet has a different base, was perceived by Warner in the structure of the present caste system in the American South. He says:

> But a new social system in Old City and its country side began to evolve out of the destruction of the old. . . . The new system continued to place all Negroes in inferior positions and all whites in superior positions. It gave certain of the family groups among the whites superior positions to all other whites, and certain family groups the most inferior and least desirable places within the white society.
>
> As the years have gone by, this new social system has become less like the old master-and-slave system out of which it was created.[6]

We may now say that when contemporary social structures, relationships, or systems bear a resemblance to old ones they may

foster ideas similar to old ones. The tendency in such cases will then be to mentally leap over the contemporary situation and hark back to the old one. This is a cultural trait of ours. It is as surely a folk idea as the insistence in our culture that children be properly trained in decent toilet habits. Thus, when the desire to hark back to the past overcomes disciplined scientific analysis we have spurious continuity—an intellectual construct—as distinguished from true continuity, which is a social reality.

JAPANESE LABOR: A TEST CASE

The transformations that have taken place in the relationships between labor and management in Japan since the introduction of industrial capitalism provide an excellent and pertinent context in which to examine the problem of discontinuity for the following reasons:

1. The structure and processes of industrial capitalism as a social form can be defined with reasonable clarity.
2. We can observe the same structures and processes operating in cultures other than the Japanese, and so can be reasonably certain that the structure as a whole has certain constant characteristics, even though in specific circumstances labor-management relations may be and are affected by the local "cultural pattern."
3. Rapid changes are taking place in Japan at the present time in labor-management relations.
4. In contemporary social science thinking there is an exaggerated tendency, often based on very little evidence, to refer present Japanese behavior to feudal times.
5. We have excellent documentation for the period in which Japan was transformed from an agrarian to an industrial economy.

Industrial capitalism everywhere is characterized by the following:

1. Urbanization.
2. Increase in number of factories.
3. Increase in industrial employment.
4. Increase in mechanization.

These conditions were as characteristic of Japan as they were of other countries into which the system of industrial capitalism was introduced. I will attempt to show that the system has had far-reaching consequences on Japanese social organization; that the consequences were to create a true discontinuity in Japanese social organization, and that consequently labor-management relations in Japan in the prewar and the postwar periods are to be interpreted largely in terms of the industrial-capitalist system of labor-management relations rather than in terms of the old feudal patterns.

In urging this interpretation I am, of course, not unmindful that most of the argument has been on the opposite side. Thus, in speaking of the relationship of the "feudal clan" to Japanese industry, the United States State Department says: "A similar paternalistic attitude dominated the relations between the larger employer and his factory hands. A subsistence income in bad times as well as good was virtually assured to labor by employers."[7]

Possibly the best way to begin the discussion of the changes that have occurred in labor-management relations in the past fifty-odd years in Japan is to give some data illustrating what occurred in the culture as a whole (Table 1).

It will be seen from this table that Japan displays characteristic features of industrial-capitalist cultures: marked increase in spatial mobility and urbanization, decline of the rural population, increase of the number of industrial wage earners, increase in mechanization. By 1940, 20 percent of Japan's population lived in her six great cities.[10]

Although it is true that many Japanese workers were employed in small establishments, the number employed in large ones even in the 1930s is impressive. Teijiro Uyeda shows that in 1935 the overwhelming bulk of the workers in the cotton-spinning industry were employed in factories having more than 100 operatives. In 1932, 72.7 percent of the workers in the rubber-boot and -shoe industry were employed in establishments having a hundred or more workers. In the electric lamp industry in 1935 more than half the workers were in factories having more than 100 employees.[11] In important strategic industries like mining, railroads, automobiles, aircraft, and shipbuilding large pay rolls were, of course, common.

Japan thus quickly adopted, or became subject to, important overt features and internal processes of industrial capitalism. How close was this union of "old Japan" with the economic system of the West is

indicated by the fact that as early as 1897 Japan was struck by periodic financial and industrial crises similar to those of the United States and at almost exactly the same time! Thus Japan's crises occurred in the following years: 1897, 1900, 1908, 1914, 1920, 1923, 1927 and 1929–30.[12] This series of crises paces our own practically step by step.[13]

It seems reasonable, now, to expect that this vast and sudden urbanization, this flocking of millions into the factories and sweatshops of Japan, could not but unhinge old relationships that were based on an economy overwhelmingly agrarian, largely farm-tenant, and oriented toward primary relationships. This hypothesis appears even more reasonable when it is borne in mind that the process of industrialization was accompanied not only by the adoption of Western mechanical techniques, but also by an influx of Western ideas.[14]

What, then, were the consequences in terms of human relationships, of this change of great magnitude? In accordance with the stated objectives of this section I shall attempt to illustrate what happened through an analysis of labor-management relations.

The social organization of industrial capitalism is characterized everywhere by the following:

1. There are two groups of persons: the employers and the employed.
2. The employers own the means of production, the employees are paid money to operate them.
3. The employer tries constantly to maximize the return on his investment and minimize costs.
4. The employees strive constantly to maintain what they consider a reasonable balance between their money earnings and the cost of living.
5. A system of laws comes into existence the purpose of which is to regulate the relations between management and labor.
6. These laws are enforced by police power.

The operation of these factors has, in capitalist cultures, whether through internal development or through diffusion, been accompanied by the following:

1. Conflict between labor, management, and government.
2. Development of labor unions.

Japan did not enter the capitalist structure of labor-management relations at a single bound. Rather, the new structure was erected on the battered foundations of the feudal order. The first large-scale factory in Japan, a cotton-spinning works with 5,000 spindles, was set up by the feudal lord of Satsuma in 1862. Other early factories were established by the feudal chief of Mayebashi and by Nabeshima, the old feudal lord of Saya, in 1870.[15] In the early days of industrialization, labor-management relations appear to have been colored by *kazoku seido* ("family system"), in accordance with which the employer, out of his *ninjo* ("humane feelings"), was presumed to look after the well-being of his employees. The rapid industrial expansion after the Sino-Japanese war, however, resulted in the development of

TABLE 1

SOME ECONOMIC AND DEMOGRAPHIC CHANGES IN JAPAN 1898–1940*

Percentage of population living in places of 10,000 or less in 1898	82
Percentage of population living in places of 10,000 or less in 1940	50[8]
Number of towns of 10,000 or more inhabitants in 1898	153
Number of towns of 10,000 or more inhabitants in 1925	609
Increase in numbers of factories per year, 1868–98	100–300
Increase in number of factories in 1907	1,000
Increase in number of factories in 1917	2,000
Number of factory workers in 1909	800,000
Number of factory workers in 1931	2,032,353
Number of industrial workers in 1931	4,729,436
Number of industrial workers in 1940	10,094,000[9]
Percentage of factories using electric power in 1909	28.3
Percentage of factories using electric power in 1929	83.2

* Statistics from *Industrial Labour in Japan.* (Studies and Reports, Ser. A, Industrial Relations, No. 37). Geneva: Int. Labour Off. 1933.

a new class in Japan: the industrial wage earner. Meanwhile, the problems of competitive enterprise, on the one hand, and the rising cost of living in the cities, on the other, placed heavy burdens on worker earnings. At the same time, the recurrent industrial and financial crises resulted in widespread poverty and unemployment. These were situations of such magnitude that the old family system could no longer cope with them. In this connection the words of the International Labour Office are portentous:

> Capitalistic economy which has brought into existence a large class of wage earners . . . has led to the transformation of the old friendly relationship between the *oya-kata* and his *dei-shi* (at once pupil and worker) into an impersonal relationship between the employer and employed. . . .[16]

With the influx of new ideas from the West the stage was set for a change in the structure of human relations; the stage was set for industrial disputes. Between 1919 and 1930 the number of disputes involving strikes, lockouts, or slowdowns doubled. One of the most important strikes occurred precisely in a firm in which the family-system type of operation was most strongly exemplified: the Kanegafuchi Spinning Company, which employed 35,000 workers in thirty-six mills. This concern provided its workers with dormitories, educational and recreational facilities, health services, illness and invalidity allowances, and death benefits. Nevertheless, when the company attempted to cut the cost-of-living bonus, a strike ensued.

As in all young labor movements, the first strikes were poorly organized and not very successful. Nevertheless, the railway strike of 1898 was carried through under strict discipline and paralyzed the right of way of the Nippon Railway Company. After a period of police suppression of the labor movement beginning in 1900, a new series of strikes broke out during the first world war. In 1919 the slowdown was used in twenty-three strikes. The period 1920–30 was characterized by growing discipline in organizing strikes. Of the depression period of 1920, the International Labour Office states: ". . . any strike that broke out was serious, for it was part of organized warfare, with carefully planned tactics and a 'technique' of its own." Concluding its survey of ten years of Japanese strikes, the same source says: "This survey of the larger strikes of the last 10 years suggests the increasing tension of industrial relations in Japan."

Japanese labor organization operated constantly in the shadow of police power and restrictive legislation, culminating in its ruthless suppression in 1938. However, the report of the United States Strategic Bombing Survey points out that the Japanese were never able to eliminate entirely strikes or walkouts. They did, however, succeed in reducing the number from 628 cases involving 127,730 participants in 1937 to 216 cases with 6,627 participants in 1944.[17]

HAVING established the growing resemblance of the Japanese labor-management relations pattern to the labor-management relations pattern in other capitalist countries, we may now proceed to examine some recent interpretations of the relationship of "feudalism" in Japan to labor-management relations in that country. We shall examine the interpretations of those who, through failure to recognize the discontinuity that has developed in human relations in Japan, continue to evaluate the situation in terms of ancient institutions. I should like to emphasize in this connection that my disagreements with the interpretations I shall cite are not of a dogmatic character. In a changing situation it is difficult to assert with absolute assurance that "this is of the old" or that "this is of the new." What I should like to suggest, however, is the real importance of the alternate hypothesis for the understanding of contemporary Japan.

Example 1. "The wage structure [of Japan] reflects the feudal philosophy of wages, aiming not so much at compensation for the productive effort as at assuring the subsistence—on a very low level—of the employee and his family, and at rewarding him for his loyalty to the employer."[18]

Alternate hypothesis: The aim of employers in a capitalist economy is to reduce costs to a minimum. Low wages in capitalist countries have been one of the principal problems of the capitalist economy. Hence, the

situation in Japan reflects the general-capitalistic rather than the specific-Japanese.

On the other hand, it can be no news to the technicians of the United States Bureau of Labor Statistics that estimates of worker income anywhere should include income other than wages. In this connection, the United States Strategic Bombing Survey states that in addition to wages, Japanese workers received extra money for efficiency, overtime, perfect attendance, and family obligations.[19] This form of supplementary payment is common the world over. In a certain sense, the United States is unique in the degree to which supplementary allowances are not an important part of worker income.

The article cited contains a number of unproved assumptions of this kind: Thus, in explaining the "absence of any provision for special procedures" in certain circumstances in Japanese labor agreements, the article says: "This omission might be explained by a traditional Japanese inclination to avoid formal decisions about right and wrong. . . ." The explanation of low wages paid to women is "the subordinate position which women traditionally have in Japan." Again, "The basic human rights of workers have never, because of Japan's feudal and paternalistic background, been properly recognized or protected." But absence from labor-management agreements of "special procedures" is characteristic of young labor movements; low wages have been woman's lot in many places in the world, and the "basic human rights" of workers were scarcely recognized even in the United States before the days of the N.R.A. On the other hand, they were recognized ten years earlier in Chile, which is still much closer to feudalism than we are!

Example 2. Occasional "voluntary offers" by Japanese workers to take pay cuts in times of depression are "no doubt a survival of the feudal Spirit in Japanese industry. . . ."[20]

Alternate hypothesis: In times of depression it is necessary for the employer to reduce costs. Hence, the acceptance by labor of wage cuts is a reflection of the general-economic rather than of the specific-Japanese.

The following quotation from the *New York Times* of August 10, 1947, is of particular interest. The article bears a Rutland, Vermont, dateline and under the head WORKERS TAKE CUT TO SAVE RAILROAD makes the following statement:

> The employees of the line have taken cuts before. In 1941 they accepted a 5 per cent deduction for two years as a "voluntary contribution of the employees for the purpose of improving the financial condition of the Rutland (Vt.) Railroad."

The statement in the International Labour Office report is particularly interesting because of the words "voluntary" and "offer." Conversation with officials of the American labor movement and of the United States Department of Labor suggests that although wage cuts were accepted by American workers during depression periods, the closest we get in this country to an "offer" by American labor to cut wages is the open negotiation of agreements. Closer examination of the Japanese situation, however, shows that even the occasional "offers" were not quite as deeply rooted in the soil of old Japan as they may at first blush appear to be. In the last sentence of a footnote referring to this question is the following: "In another case the workers at a certain knitting works in Nagoya made a proposal to accept a 10 per cent reduction, but with a view to forestalling the company's announcement of a cut of 20 per cent." Thus, what the situation appears to boil down to in Japan is the occasional special phrasing of the unpleasant situation.

Example 3. "Industry generally employs two or three times too many persons because of the ancient philosophy that the boss is responsible for the welfare of his workers."[21]

Alternate hypothesis: Postwar paralysis in enemy countries, uncertainty, unrest, material shortages, inflation, and physical weakness of labor from hunger make it inexpedient for management to aggravate the crisis by wholesale discharge of workers. At this point it is important to observe two facts: The practice of maintaining pay rolls far in excess of requirements is characteristic of postwar Europe, especially Germany;[22] the plan for laying off workers intensified unrest in Japan, according to the article cited, and is about to precipitate the "end" of the "truce." The article in the *Times* states that "the plan for dropping useless workers are [*sic!*] making the work of agitators far easier." Even

without the parallel instances from European experience, it would be possible to argue from the Japanese situation alone that the appearance of unrest in the face of the proposed layoffs is in itself proof that the maintenance of excessive pay rolls was intended to prevent unrest.

It is important to bear in mind that government control over the labor force, which grew as the Asiatic venture moved toward ultimate catastrophe, hastened the depersonalization of employer-employee relationships, and the undermining of the old patterns.

WHO seeks for "old"—for "feudal"—Japan in the Nippon of today seeks a ghost among ashes. The old order, reflected so sensitively in the work of Lafcadio Hearn, was disappearing even before our iron birds made rubble of Japan's great cities. For Japan, having adopted the machinery of industrial capitalism, was being changed by it. What was taking place in Japan was not so much the transformation of capitalism by the Japanese "cultural pattern," as the anthropologists love to put it, but rather the transformation of Japan by a new cultural entity introduced from across the sea. And this, it seems to me, is the situation today: it is not so much that old, local "culture patterns" are determining what changes shall take place in the world today, but rather that irresistible forces are pushing the world toward a goal that shall be very much the same for many of us regardless of our traditions. Wherever we go, wherever we shall arrive, will be a place of no return. For the old ways will be gone, and the changes that occur will bring about a new alignment of human relationships. This alignment will be for us a discontinuity—a break in our "cultural tradition"—that will force us to adopt new ways of thinking about ourselves and the world if we wish to survive in all sanity.

REFERENCES

1. WOODWARD, E. L. Europe is Worth Saving and Can be Saved. *N. Y. Times Mag.*, 1947, July 13.
2. LAPIERE, R. T. *Sociology.* New York: McGraw-Hill, 1946. P. 462.
3. EDWARDS, R., and HOPEWELL, M. D. *Edwards' Great West.* St. Louis, Mo.: Trubnor & Company, 1860. (Pub. at the off. of *Edwards' Monthly*, a Journal of Progress.).
4. ODUM, H. W., and MOORE, H. E. *American Regionalism.* New York: Henry Holt, 1938. P. 473.
5. GROVES, E. *The American Woman.* New York: Greenberg, 1937. P. 83.
6. DAVIS, A., BURLEIGH, B., and GARDNER, M. R. *Deep South.* Chicago: Univ. of Chicago Press, 1941.
7. VERNON, R., and WACHENHEIMER, C. *Bull. U.S. Dept. of State*, 1947, 17, 419.
8. Japan. *Encyclopaedia Britannica.* (14th ed.)
9. The Japanese Wartime Standard of Living. *U.S. Strategic Bombing Survey.* Washington: U. S. Gov. Print. Off., 1947.
10. Japan. *Encyclopaedia Britannica.* (14th ed.)
11. UYEDA, T. *The Small Industries of Japan.* New York: Inst. of Pacific Relations, 1938.
12. *Industrial Labour in Japan.* (Studies and Reports, Ser. A, Industrial Relations, No. 37). Geneva: Int. Labour Off., 1933.
13. LANDIS, P. *Social Policies in the Making.* Boston: D. C. Heath, 1947. P. 375.
14. *Industrial Labour in Japan.* (Studies and Reports, Ser. A, Industrial Relations, No. 37). Geneva: Int. Labour Off., 1933. Pp. 45–46.
15. *Industrial Labour in Japan.* (Studies and Reports, Ser, A, Industrial Relations, No. 37). Geneva: Int. Labour Off., 1933.
16. *Industrial Labour in Japan.* (Studies and Reports, Ser. A, Industrial Relations, No. 37). Geneva: Int. Labour Off., 1933.
17. The Japanese Wartime Standard of Living. *U. S. Strategic Bomb. Survey.* Washington: U. S. Gov. Print. Off., 1947.
18. WEIGERT, O. *Monthly Labor Rev.*, 1947, Feb.
19. The Japanese Wartime Standard of Living. *U. S. Strategic Bomb. Survey.* Washington: U. S. Gov. Print. Off., 1947.
20. *Industrial Labour in Japan.* (Studies and Reports, Ser. A, Industrial Relations, No. 37). Geneva: Int. Labour Off., 1933.
21. *New York Times*, 1947, July 23.
22. *Manpower, Trade Unions and Working Conditions* (Monthly Rep. of Military Gov., No. 9, 1946, April 20). Military Government of Germany.

SCIENCE ON THE MARCH

CENTRIFUGAL FIELDS

THE generalization of Newton that every uncharged macroscopic piece of matter attracts every other uncharged macroscopic piece of matter in the universe is perhaps one of the most widely applicable laws of physics. Newton further showed that the amount of this attraction F between any two homogeneous spherical masses M_1 and M_2 a distance R apart is given by the simple relation

$$F = G\frac{M_1M_2}{R^2}$$

where G is a universal constant. It will be observed from this relation of Newton that the gravitational force of the earth on any body near its surface is proportional to the mass of the body and to the mass of the earth. Since the force on a body is equal to the mass times the acceleration, every freely falling body near a given point on the earth's surfaces falls toward the earth with the same acceleration. This acceleration is called the acceleration of gravity g, and, although it varies slightly over the earth's surface, g is about 981 cm./sec^2. From various observations, astronomers have determined the gravitational acceleration on the surfaces of a number of astronomical bodies. For example, on the moon the gravitational acceleration is about one-sixth g, and that on the sun is almost 28 g. The most intense gravitational field so far found by the astronomers is 80,000 g on the surface of the star van Maanen. In other words, a mass weighing 100 pounds on the earth would weigh 4,000 tons on van Maanen if the respective weighings were carried out on spring balances. So far, man has not been able to change appreciably the gravitational acceleration of the earth or any of the astronomical bodies. He has, however, found ways of exerting forces and accelerations that are equivalent to gravitational accelerations and forces in the way matter is affected. For example, such forces are experienced whenever a body is stopped or started or when it has its direction of motion changed. The forces resulting from the latter are, in practice, more akin to gravitational forces than any of the others, since they can be maintained constant for comparatively long periods of time.

If a mass M is whirled around an axis with an angular speed of N revolutions per second, the centrifugal force $F = 4\pi^2N^2\ Mr$ where r is the distance of M from the axis of rotation. If one takes a value of $r = 1$ meter, then the value of N required to give an acceleration equal to that of gravity g on the earth is only 0.5 rev./sec. Consequently, we see that it is possible to produce centrifugal forces up to values many times that of gravity; also, that they may be maintained over comparatively large regions or volumes. Perhaps the most valuable use so far found for centrifugal forces is the separation and purification of various materials. It is common knowledge that, if particles of various sizes and densities are allowed to settle out of a fluid, the larger, heavier particles settle out first, followed in turn by the smaller or less dense ones. Just over a century ago, Sir George Stokes showed that the velocity of fall or sedimentation of a spherical particle with a density d_p and radius a is given by the relation

$$v = \frac{2}{9\,n}(d_p - d_l)\,a^2F$$

where d_l is the density of the liquid, n its coefficient of viscosity, and F the gravitational or centrifugal force. Consequently, the larger the value of the force F, the higher the settling speed. Upon examining the above equation, it might seem that from a practical standpoint all separation due to sedimentation could be carried out in the earth's field rather than in a centrifugal field, as the only advantage of the centrifugal field

would be in the shorter time of settling, and this could be compensated for by using larger settling tanks. However, this turns out not to be the case. It will be observed from the equation that the speed of settling decreases very rapidly as the size of the particle is decreased, and becomes extremely small when the particle approaches molecular dimensions. It is well known that all particles experience an ever-present Brownian motion, the amplitude of which increases as the size of the particle decreases and becomes quite large when the particle approaches molecular dimensions. This Brownian motion, or diffusion, always transports particles from regions of higher concentration to regions of lower concentration and hence opposes or stops sedimentation unless the centrifugal or gravitational force is large enough to overcome it. Consequently, in a great many important applications, the larger centrifugal forces, instead of gravity, must be used to produce any appreciable sedimentation, and as a result centrifuges or apparatus for producing centrifugal fields are common tools both in industry and in the laboratory.

As shown above, when the particle size reaches molecular dimensions, the centrifugal field must be made very large to produce sedimentation. However, with modern technique, sufficiently large centrifugal fields can be obtained to produce molecular sedimentation and, in fact, to separate the isotopes of the elements. Fortunately, most of the biologically important substances, such as viruses and hormones, are not deactivated by centrifugal fields large enough to concentrate them, so that the centrifuge method is a most effective way of purifying them. Since the velocity of sedimentation in a centrifugal field depends upon the size and mass of the particles being sedimented, the centrifuge may be used for determining both the mass and size of the particles.

If the particles are molecules, their mass and size also can be determined, and it turns out that the ultracentrifuge method, first developed by Svedberg and his colleagues about twenty-five years ago, is perhaps the most reliable way so far devised for measuring the molecular weights of proteins and many other types of high molecular weight compounds. Also, if the particles are not of uniform size, the distribution of their masses and sizes can be obtained by centrifuging. It is interesting to note that the general centrifuge method has been used for obtaining molecular weights over the entire known molecular weight range, i.e., from hydrogen, the lightest known molecule, to certain biological compounds with molecular weights in excess of twenty million molecular weight units.

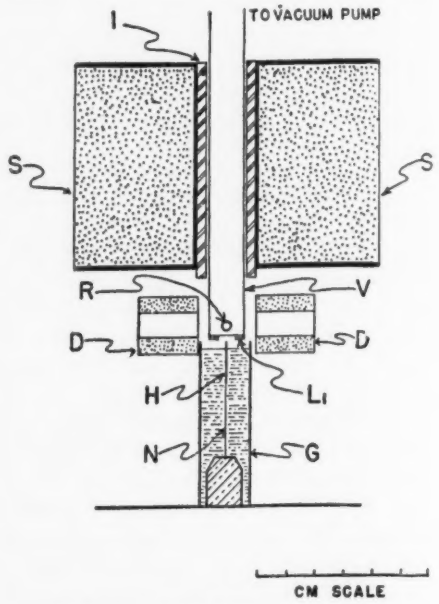


FIG. 1. ULTRACENTRIFUGE

SCHEMATIC DIAGRAM OF APPARATUS FOR PRODUCING ULTRAHIGH CENTRIFUGAL FIELDS. THE FUNCTION OF THE PARTS LETTERED IS GIVEN IN THE TEXT BELOW.

In most of the experiments, where molecular sedimentation is obtained, the centrifugal field is in the range from 10^4 to 10^6 gravity which, except near the surface of a few distant stars, is much larger than any of the gravitational fields found so far in nature. Also, H. W. Beams, the Harveys, and others have found this same range of centrifugal fields most effective in their studies of the displacement of the components in biological cells. However, it has been found that the above maximum centrifugal fields of a million times gravity are far too small for

effective use in a number of different experiments, especially in physics. Consequently, a search for new ways of producing higher centrifugal fields has been undertaken at the University of Virginia, with the result that much higher centrifugal fields are available. For example, centrifugal fields of the order of half a billion gravity are now possible for laboratory use.

The technique of producing these high fields, although comparatively simple, will not be outlined in detail here, as it has been adequately described elsewhere. It might be of interest, however, to indicate briefly the general principles of the method. Figure 1 shows a schematic diagram of the apparatus. The rotor R is made of steel or other ferromagnetic material and is supported by the axial magnetic field of the solenoid S. It is well known that such a suspended rotor is stable as far as its lateral motion is concerned, but is not stable vertically if the current through the solenoid is steady. In order to give the rotor vertical stability, a small pickup coil L placed either below or above the rotor is made to regulate the electrical current through the solenoid S; i.e., if the rotor R moves slightly upward, the current through S is decreased, whereas if it moves downward, the current through S is increased. Electrical damping also is introduced into the circuit so that, in practice, no vertical or horizontal oscillation of the rotor can be observed when it is viewed with a 30-power magnifier focused on scratches on the rotor surface. A small iron wire H, mounted in a glass tube filled with a liquid, assists in damping any horizontal motion of the rotor. The rotor is surrounded by a glass vacuum chamber V, which can be highly evacuated to reduce the gaseous friction. The rotor is spun by a rotating magnetic field produced by an alternating current through four coils D placed symmetrically around the rotor. The rotating magnetic field is used both to accelerate and decelerate the rotor. The speed of the rotor is measured by a photoelectric pickup arrangement.

It will be observed that the magnetic field that supports the rotor is symmetrical with respect to the axis of rotation. Consequently, there are no appreciable eddy currents induced in the rotor that can resist its rotation, so that the only forces which substantially oppose its rotation arise from gaseous friction on the rotor surface. Since the air pressure in V can be made very small, the frictional drag can be made correspondingly small. For example, with a 1.59-mm. spherical rotor spinning 100,000 r.p.s., the driving current in the coils D was turned off and the rotor allowed to coast for a few hours while the deceleration was measured. It was found that the loss in speed was only about 0.1 percent per hour when the gaseous pressure surrounding the rotor was about 2×10^{-6} mm. of Hg. This value is in accord with that calculated from the kinetic theory of gases and in fact suggests that such spinning rotor arrangements can be used as absolute pressure manometers for measuring low gaseous pressure.

Test experiments were carried out with a series of small hardened-steel spherical rotors to determine the rotational speed necessary to make them explode. Table 1 shows some of the results obtained. It will be observed that the maximum peripheral speeds obtained were roughly constant (10^5 cm./sec.) for all the rotors. This should be expected from theory if the steel were perfectly elastic. On the other hand, the maximum centrifugal acceleration obtained was much larger with decreasing size of rotor and became 428 million times gravity for the 0.521-mm. rotor.

TABLE 1

Diameter Rotor	Rotor Speed	Peripheral Speed	Centrifugal Acceleration × Gravity	Maximum Calculated Stress
mm.	r.p.s.	cm./sec.		lb./in.^{2}
3.97	77,000	9.60×10^4	4.71×10^7	410,000
2.38	123,500	9.25×10^4	7.20×10^7	385,000
1.59	211,000	1.05×10^5	1.43×10^8	498,000
0.795	386,000	9.65×10^4	2.40×10^8	420,000
0.521	633,000	1.04×10^5	4.28×10^8	488,000

The very low frictional torque on the spinning rotor obtained in practice with the technique outlined makes it possible to obtain extremely constant rotor speed. In fact, it is

found that the rotors may be driven in synchronism with a piezoelectric crystal. This type of drive maintains the speed as constant as the frequency of the crystal-controlled driving circuit and should be better than one part in ten million. When mirror faces were ground on the magnetically suspended rotor, no appreciable friction was introduced, so that a rotating mirror of extremely constant speed was obtained. This development makes possible many experiments other than those requiring ultrahigh centrifugal fields. For example, the precision with which the speed of light can be determined may be increased by a factor of roughly 100 if the light path could be measured with the same precision as the rotational speed of the mirror. Incidentally, experiments are already in progress at Virginia for increasing the precision of measurement of the light path in comparable degree with that attained in measuring the speed of the rotating mirror by measuring the light path in terms of the wave length of a given spectral line. The method makes use of an interferometer somewhat similar to that used by Michelson many years ago for measuring the length of the standard meter in terms of the wave length of light except that a photoelectron-multiplier cell and scaling circuit are used, instead of the human eye, for counting the fringes. The low frictional torque on the rotor also makes possible many other experiments such as the measurement of the pressure of light, etc.

From both theory and experiment (Table 1) it is evident that, as the diameter of the rotor is decreased, the maximum centrifugal force produced before rotor explosion increases. Also, with the technique of spinning the rotors discussed above, smaller rotors obviously may be spun than is indicated in Table 1. As a result, much higher centrifugal forces can be obtained. However, the successful study of the general effect of these high centrifugal fields on the properties of matters depends upon the development of methods of observing such changes in the fields. Consequently, considerable effort in the future must be directed to the study of these problems.

J. W. BEAMS

Rouss Physical Laboratory
University of Virginia

TENTATIVE ITINERARY*

FIRST ANNUAL INTERNATIONAL PHOTOGRAPHY-IN-SCIENCE SALON

THE BUHL PLANETARIUM AND INSTITUTE OF POPULAR SCIENCE Pittsburgh, Pennsylvania	January 10–24	GAMMA SIGMA EPSILON University of Florida Gainesville	May 13–June 1
CRANBROOK INSTITUTE OF SCIENCE Bloomfield Hills, Michigan	February 2–28	THE JOHN CRERAR LIBRARY Chicago, Illinois	June 8–June 21
CLEVELAND HEALTH MUSEUM Cleveland, Ohio	March 8–29	AUBURN CAMERA CLUB Bureau of Animal Industry, U.S.D.A. Auburn, Alabama	June 28–July 12
Science Illustrated New York City	April 6–16		
BROWN UNIVERSITY Providence, Rhode Island	April 22–May 6	AMERICAN OSTEOPATHIC ASSOCIATION ANNUAL CONVENTION Boston, Massachusetts	July 19–23

* Definite dates will be published later. Reservations for showing the 1948 Salon pictures may be made now.

BOOK REVIEWS

THE GRENFELL COUNTRY

Outlines of the Geography, Life and Customs of Newfoundland–Labrador. V. Tanner. 2 vols. 909 pp. Illus. $13.50. Cambridge Univ. Press, Cambridge, Eng. Macmillan. New York. 1947.

DR. TANNER, with a broad grasp of geology and geography, has prepared a most interesting report on his field studies in the southeastern part of Labrador adjoining Newfoundland, but separated from that island by the Strait of Belle Isle. Associated with Dr. Tanner during his expedition to Labrador in 1937 were Dr. E. H. Kranck, a geologist and geographer colleague at the University of Helsingfors, and Dr. I. Hustich, a botanist from Helsingfors. These three men were experienced fieldworkers and were particularly well acquainted with the geology and botany of the Fennoscandia. They were inspired to carry out as far as they could, comparative studies in the physical environment of two areas in about the same latitude but on opposite sides of the North Atlantic Ocean.

In geologic structure there are many similarities in the two areas. Both regions are composed primarily of ancient rocks that are part of the Pre-Cambrian Complex. Both have been severely glaciated. They abound in lakes, ponds, and marshes and have, because of glaciation, disarranged drainage systems. Both areas have extensive forests, but in Labrador the forests do not come down to the east coast, where most of the inhabitants live, nor do they extend to the northern part of the peninsula of Labrador. In climate, population-carrying capacity, in natural resources and cultural development, the two areas are distinctly dissimilar.

In the second expedition, made in 1939, there were six American students who, because of their field studies in geology and geography, served as assistants. The work of this expedition was suspended owing to World War II, and Dr. Tanner, although he has written a very full and excellent report, emphasizes that his work is incomplete and that we must view these volumes as a report of progress.

After treating in detail the discovery and mapping of this land, which he believes is the first land seen in the new world by Europeans, the author presents in Part I a somewhat detailed treatment of the geomorphology of this little-known part of the world.

Over the surface of a Pre-Cambrian Complex, which is part of the Canadian Shield, there is an old-age stream erosion surface, a peneplain, which was uplifted late in Tertiary time. This old erosion surface is represented today by many gently rolling or nearly flat uplands. The valleys, developed since the peneplain was uplifted, had reached a stage of late youth when the great ice sheets of the Quaternary period advanced over this country.

The recurring invasions of ice carried away the old soils and smoothed many of the bare rock surfaces. The irregular deposition of glacial debris blocked the drainage and caused the development of the innumerable lakes, ponds, and marshes that give character to the landscape.

The Torngak mountains are in the far northern part of the peninsula and are due to uplift and block-faulting of the peneplain. At the southern margin of Labrador, bordering the St. Lawrence River, block faulting and uplift are recorded. The coast shows evidence of depression and drowning. There are numerous estuaries, and many of them have the form of small fjords. It is a coastline dotted with many islands, which can be accounted for by the sinking of the land relative to sea level.

Dr. Tanner believes that the submarine valleys off this coast were made by streams when the land was high. He does not enter into a detailed analysis of the various theories that have been advanced for the submarine

canyons in the continental shelves of the world. He concurs with many other observers that the marine terraces and beaches along the Labrador coast are due to climatic changes brought on by the melting of the great ice sheets and the consequent removal of weight upon that part of the continent. The upward movement of the land was, in Dr. Tanner's judgment, slow and broken by brief halts. The periods of cessation in the upward movement gave the waves an opportunity to cut marine terraces and to clear away the fine debris on the land surface along the beaches. Lines of boulders that could not be easily moved by the waves are left at the inner edges of many of these beaches and are interpreted by many observers as shore-line features.

Definite indications have been observed of at least four separate ice advances. Distinct sheets of moraine have been found separated by stratified sands and peaty beds in the Hudson Bay area. The members of the expedition made many excursions far into the heavily forested interior of Labrador, and detailed accounts of their findings are recorded.

From his studies in northwest Europe and in North America, Dr. Tanner now estimates that the evaporation of sea water to form the great continental ice sheets of the Pleistocene period may have lowered the ocean about 240 feet. The return of the water to the sea as the ice melted should have caused a corresponding rise in sea level. Others have estimated that these changes in the relative position of the sea and the land may have amounted to as much as 300 feet. Dr. Tanner finds a close analogy between the raised beaches of Fennoscandia and those of the Labrador coast. He came to the conclusion that uplift is still taking place in both areas and, therefore, that readjustments following the ice age are not yet completed.

In Part II, entitled The Sea and the Labrador Current, there is a very full and careful presentation of all the known data on the configuration of the bottom of the sea immediately east of Labrador and the temperatures of the waters in that area.

In Part III, dealing with meteorology and climate, the author has analyzed with great care the continental and oceanic influences, giving due consideration to the more or less permanent low-pressure area over the Atlantic Ocean south of Iceland and the high-pressure area in the far north of that ocean. The many changes in weather from day to day he accounts for by the cyclonic storms coming chiefly from the southwest.

In his skillful and well-informed comparative studies of climates he makes the following exceedingly clear statement:

> On the one side we have the great heat regulating centre of the North Atlantic, the Gulf Stream and its continuation, the warm North Atlantic Current on the European coast, and on the other, off the American coast, we have the cooling apparatus, the icy Baffin Land Current and its continuation, the Labrador Current. If the functions of these two systems were suspended, the isotherms would be transported a thousand kilometers southward on the Fennoscandian coast and as much northward on the Labradorean.

Part IV deals with plant life. The northern portion of the Labrador peninsula is a part of the arctic tundra zone. That is a treeless landscape with grasses, mosses, and shrubs. The northern limit of trees crosses Labrador in latitude 55° north, which means that the whole Torngak mountain area and near-by coast is in a tundra belt. South of that belt there is an area of small coniferous trees without noteworthy economic value. The zone of small trees and bushes extends along the entire eastern coast of Labrador to the Strait of Belle Isle. The southern portion of Newfoundland Labrador, except near the coast, is clothed with a high forest of coniferous trees of distinct economic value, but it includes many swampy lands and barren rock surfaces.

Part V is devoted to the animal life of Labrador. These expeditions did not have professional specialists in zoology, and no claims are made for any startling discoveries in this field. Some of the descriptive statements in the report are most impressive, however. The forest observers in Dr. Tanner's party and many others who have visited this region have been much impressed by the absence of animal life. In much of Labrador, one writer reports he "was surprised by the awful silence of the forests." Another states:

> Long and anxiously I looked around in every direction to see if I could distinguish any signs of animal life, but without success. No sound was audible except the wind. . . . Not a bird, or butterfly, or beetle appeared to inhabit this desolate wilderness; the silence of the lifeless surroundings depresses the spirit and saddens the heart.

In spite of these reports, the Hudson Bay Company, which deals primarily in furs, has had stations established in Labrador for at least 200 years.

Part VI, dealing with Human Life, occupies the entire space of Volume II. After a very thorough and scientific analysis of the physical and biologic setting in this bleak environment, this geographer and his associates have appropriately turned to the study of man. No geographer should attempt to interpret the ways of living in any part of this world without first making such an analysis of the environment. Geography is a study of great human dramas, and the topography, climate, animal, and vegetable life present are parts of the stage settings where the human dramas are enacted. The actors are interesting and important in themselves, but their adjustment to the environment provides the substance for the geographer's field of study. His interpretations and predictions should become important and significant if he has followed faithfully a scientific method of work. No science has yet been developed and proved to be of great human value that has not the power of predictability.

Evidently Dr. Tanner has a keen interest in human geography. With numerous maps and photographic illustrations he has brought before us the location of the villages, the distribution of the areas occupied chiefly by the Eskimos, the white people, and the Indians. The permanent habitations in this land are always at the seashore. No dwellings have been set up out of sight of the high-tide lines. More than half the total population of 4,716 (based upon the census of 1935) are found south of latitude 54°, which means that they are concentrated along the southeast coastal area bordering and immediately north of the Strait of Belle Isle. That is the region where 90 percent of the white people live, and it is the region where the Sir Wilfred Grenfell missions were established.

We must recognize that the Newfoundland Labrador land is a primeval wilderness with the exception of a few widely scattered camping grounds in the interior and the few inhabited spots on the coast. For seven or eight months of each year a belt of coastal ice encloses this deserted land, completely separating it from the rest of the world.

The three most important ethnic groups in the population are the Eskimos, Indians, and the white people. The Eskimos occupy their natural home on the bleak coastal tundra and the neighboring strips of woodlands. Some of them live on the islands or have placed their little homes on the shores of fjords.

The Indians make their homes in the forest lands of the south, where the labyrinths of lakes and the network of rivers provide them with a habitat to which they are adapted. The distribution of the Indian population and the division of these people into various bands are presented in a map, from which it is clear that they, for the most part, avoid the tundra area. The trails of migration of these Indian bands, as well as the trapping ground of the white people, are clearly portrayed on another map.

After summarizing the work of many others who have visited Labrador and described the white people, Dr. Tanner presents his observations and describes in a very clear way the conditions of living along that bleak northern coast. There are many personal references in his reports to those who have given their lives as missionaries and social workers. He is exceedingly enthusiastic regarding Dr. Grenfell's work and also that of the late Dr. Harry L. Paddon, who served as surgeon, justice of the peace, and a "Great Gentleman of Labrador" during some thirty years as a local leader in The International Grenfell Association.

At the close of the book a little space is devoted to the prospecting for mineral deposits in Labrador. There are deposits of magnetite, layers of hematite, and some siderite. The Labrador Mining and Exploring Company is still carrying on field studies; the ore, known to exist, is claimed to be sufficient in amount to provide, at some time in the future, "a great local iron production."

At the close of Volume II there is a very complete bibliography of Labrador.

In a most modest and humble way the author and leader of these two exploring expeditions claims that he has made but a beginning in a work of scientific exploration. He wants his report to serve others who may carry further the studies he has begun. He is far too modest, for he has presented in many ways a model study in modern geographical science and embodied a vast amount of significant data. He has presented this in a most readable, and at places brilliant, literary style.

WALLACE W. ATWOOD

Clark University

LIFE MORE ABUNDANT

Science in Farming. The Yearbook of Agriculture, 1943–1947. Alfred Stefferud, Ed. xiv + 944 pp. Illus. $2.00. Gov. Print. Off. Washington. 1947.

THIS volume contains 135 articles written by the scientific staffs of the sixteen major bureaus and other units of the U. S. Department of Agriculture, presenting the most important results of research in farm problems investigated during the years 1943–47. Obviously, the sheer size of the book, with its wide variety of topics, precludes the possibility of listing even the titles of all the articles within the space of this review. A cursory glance at the table of contents gives one a fresh realization, if such were necessary, of the almost limitless range of scientific questions with which these experts of the U.S.D.A. constantly busy themselves.

Recognizing, as they do, that the continuation of human life and civilization hangs upon the production of adequate food and clothing, methods of increasing the output per animal or plant unit or per acre received perhaps the most devoted attention, with results of vital concern not only to farmers but the whole consuming public. The signs of ominously increasing pressure of population growth upon the limited area of productive land on the globe lend primary importance to all conceivable means of progress along those lines.

Naturally, therefore, the book is studded with details of practicable suggestions for increasing the volume and quality of beef, milk, pork, poultry, and eggs by improved methods of breeding, feeding, and housing, together with new discoveries in the care and sanitation of farm animals and control of disease among all food-producing animals, including goats and sheep. Some of these diseases are easily transmitted to man. Their control is hence as vital to the urbanite as to the farmer.

Likewise, new bits of advance are reported in methods of increasing the flavor, nutritive value, and vitamin content of a large number of food plants, of improving their appearance and market qualities. Even the everlasting struggle with the insect pests and fungus and bacterial diseases of garden and field crops is lightened by the discovery of more efficient ways of fighting these enemies.

Neither is the matter of newly introduced crops overlooked, nor the expanding use of farm crops for industrial purposes neglected. The basic need of better methods of soil conservation is set forth in sixteen articles, illuminating that field with many fresh suggestions. New plans of tillage and new farm machinery come into the discussion. The farm home, the preservation of food, care of clothing, and the future outlook for agriculture in the life of the nation are presented with helpful hints.

Finally, the interested reader will be glad to know that an elaborate index is provided whereby he may find his way among the wealth of details and topics.

E. V. WILCOX

Washington, D. C.

CONCHOLOGY

A Field Guide to the Shells of our Atlantic Coast. P. A. Morris. xvii + 190 pp. Illus. $3.50. Houghton Mifflin. Boston. 1947.

THIS very excellent little Field Guide should find a ready welcome among the shell collectors of the eastern coast. More than four hundred exceptionally clear photographs on a black background permit rapid and certain identification. Eight color plates are included which, in general, are very good.

The descriptions of the various species are

short and concise, but not stereotyped. Mr. Morris includes in each description a sentence or two concerning the habits and distribution of the Mollusca. There is a total lack of synonymy which, for the layman, is perfect. At the back of the handbook there is a short glossary—always a help to the beginner, and a ready source of reference to the more advanced conchologist.

The discussions in the introduction, on the various classes, might have been a bit longer and more fully developed, for many users of this book will undoubtedly be amateurs who are not grounded in the fundamentals of conchology. A few minor errors on the distribution of some species have crept in, and the credit line of the Classification Chart should read "Michigan" instead of "Illinois."

Not the least in the book's favor is its size, for it fits nicely into one's pocket, as field guides should, for on-the-spot identifications. Its good illustrations and lucid descriptions fill a need long felt by all those interested in Atlantic Coastal shells.

CHARLES T. BERRY

Department of Geology
University of Connecticut

MICROCOSM

One Day at Teton Marsh. Sally Carrighar. vii + 239 pp. Illus. $3.50. Knopf. New York. 1947.

IT WAS the day of the autumnal equinox—and what a day! The author has dramatized the biology of the animal population in a skillful and delightfully literary manner. The marsh is a world in itself, and she sees it through the eyes of the otter, the mink, the scud, the snail, the mosquito, and a host of others, all leading bothered lives, each trying to eat somebody else and trying to escape being eaten.

Without making them human, Miss Carrighar has personalized her subjects. The mink proves himself a wanton killer, the beaver a substantial citizen, the otter a playboy when not worrying. One reads of the sensations of a frog about to hibernate, and of a trout who has just escaped an osprey's clutches.

Teton Marsh, near Jackson Hole, Wyoming, is the background for this study of animal life, and the marsh, as well as the pond, the brook, the river, the forest, and the very smell of approaching winter, are graphically described. It is the work of a keen and trained observer who can also write, and who understands the tragedies that occur in a community of wild things. The book is excellent natural history, presented from quite a different angle than most animal stories.

The black-and-white illustrations by George and Patritia Mattson are unusual and most attractive.

WM. M. MANN

National Zoological Park
Washington, D. C.

HISTORICAL ETHNOLOGY

Chippewa Village: The Story of Katikitegon. W. Vernon Kinietz. 259 pp. Illus. $3.00. Cranbrook Inst. of Science, Bull. No. 25. Bloomfield Hills, Mich. 1947.

THE study of a Chippewa community is in the best tradition of historical ethnology and is quite the most ambitious of the Indian studies put out by Cranbrook. The author is already an established member of the small but distinctive segment of North American anthropologists who regard themselves as ethnohistorians, whose method, here exemplified, is beginning to show its effect on the writings of historians. But it is a sad commentary on academic life, nevertheless, that during the war such an able person could be induced to heed the greater returns of commerce. While still at Michigan, Kinietz combined two years of field work with the study of documents in such a way that his book on a living Indian community reflects how earlier observers—Cameron, Cass, Gilfillan, Grant, Schoolcraft, Tanner, and Trowbridge—helped formulate his questions, which he frequently answers by quoting these authors, and sometimes by the relations of living Indian informants, or else by observations in the first person. Oftentimes, the nice detail that appears at the documentary level is confirmed at the level of his own observation, or is remarked as absent, although Kinietz noted much that was overlooked by earlier students, or simply was not

there. The chapter headings are the categories of conventional ethnography, and from them we signal out certain topics on which the treatment excels—tribal movements; economic changes (employment); social life (dances); life cycle (not birth, childhood, nor puberty, but drinking of adulthood); religion (especially contemporary spiritism); and medicine (the account of the *Midewiwin,* or "Medicine Lodge"). We have come to expect from Cranbrook Institute fine illustrations of their collections, as well as photographs from the field. The type is well designed, and the style should appeal to the general reader; for the scholar there is an adequate bibliography and index.

WILLIAM N. FENTON
Bureau of American Ethnology
Smithsonian Institution

GERIATRICS

The Years After Fifty. Wingate Johnson. xii + 153 pp. $2.00. Whittlesey House. New York. 1947.

Medical Aspects of Growing Old. A. T. Todd. 164 pp. $3.50. William Wood. Baltimore, Md. 1946.

AN INCREASING awareness both by the medical profession and the intelligent public of the urgency, magnitude, and significance of the health problems associated with senescence is to a great degree responsible for a small avalanche of new books purporting to explain senescence in nontechnical terms. Changes in population structure have made the years of later maturity more and more significant not only to the individual, but to society as a whole. The immense field of gerontology, or the science of aging, includes three major divisions: the biology of senescence, clinical medical geriatrics (concerned with the personal health of the aging and aged), and the sociologic and economic aspects of an aging population (SM, 62, 80, January 1946). These two most recent volumes are intended to help the intelligent man or woman anticipate the later years of life.

The Years After Fifty is delightful reading. The personal charm of Dr. Johnson permeates the text throughout; one listens to a cultured, mature physician discoursing frankly and sincerely on the personal problems of later maturity. His frequent citations from the classics are well chosen. The medical science is sound; the suggestions, worthy of more attention than most people will give to them. The commoner problems of chronic disease in later years are discussed clearly and briefly, perhaps too briefly. The only adverse comment justified is that the author, in a laudable and unusual effort to be brief, has committed the minor sin of omission of correlative and interpretative discussion. The book is essentially a manual of instruction and advice for individual senescents; it does not pretend to consider the biology of senescence or the broad sociologic problems. It is recommended reading.

Medical Aspects of Growing Old is of British origin; Dr. Todd is honorary physician to the Bristol Royal Infirmary. Unfortunately, he set himself a nearly impossible goal in trying to write for both a professional medical and a lay audience. The result is too simple to interest the trained professional and a bit dry for the layman. Unfortunately, also, some of the concepts presented are antiquated and long since discarded by the medical profession. The author appears unduly concerned over constipation: his therapeutic suggestions include ballet dancing for the senile! The volume is not scientifically sound, and thus cannot be recommended.

EDWARD J. STIEGLITZ, M.D.
Washington, D. C.

QUEST

The Naming of the Telescope. Edward Rosen. xvi + 110 pp. Illus. $2.50. Schuman. New York. 1947.

THIS fascinating small volume of seventy pages of text, fortified by twenty-five pages of documentation, presents Edward Rosen's analysis of the problem "Who originated the name *telescope?*" The book will be read at a single sitting, so alluring is made this quest for a word.

In following the evidence, the reader enters the colorful times when Galileo first used the "optic tube" to which he himself most often applied the term *perspicillum.* Chief wit-

nesses are members of the famous Academy of the Lynxes, and include Frederick Cesi, founder and president of the Academy. Certain important clues resolve themselves into matters of interpretation. When Cesi writes "our telescope," is he using the "imperial we" or referring to his own person? Sidelights appear on timely controversial questions. Della Porta attacks Gilbert for his "mad idea that the Earth is in motion."

At a banquet for Galileo on April 14, 1611, Cesi repeats the name telescope many times, thus "unveiling" the new term. Who invented it, the Italian prominent in scientific circles, or the Greek, poet and theologian? The answer, on present evidence, is here.

Dr. Shapley provides an appreciative foreword to this admirable detective story in miniature.

ALICE H. FARNSWORTH
Department of Astronomy
Mount Holyoke College

COSMIC CYCLES

Sunspots in Action. Harlan True Stetson. ix + 252 pp. Illus. $3.50. Ronald Press. New York. 1947.

IN *Sunspots in Action,* Dr. Stetson tells in simple language about the things that have been learned and are being learned concerning the earthly effects of changes on the sun. Sunspots, which have been recorded accurately for more than a century, furnish the best index of solar activity. Their changes have been compared with almost every conceivable variable on earth, especially anything that varies in cycles.

It has been known for many decades that the earth's magnetic field is influenced by solar changes, but these effects, which did not touch our daily lives, seemed trivial and of only casual interest except to scientists. The same solar action that influences the earth's magnetic field also affects the ionosphere, varying the efficiency of radio transmission. With the increasing use of radio waves, this subject has become of personal importance to more and more of us. *Sunspots in Action* tells how and why radio reception is affected by sunspots and their related solar phenomena. Although the book deals primarily with the various effects of sunspots on radio reception, it also includes discussions concerning their possible effects on other earthly things that touch us closely—weather and economic cycles.

Popular interest in this subject is enhanced by the fact that the sun is now exceptionally active. The largest sunspots ever observed, and the greatest number of spots on the sun at one time, were recorded in 1947. This great solar activity, coupled with our increasing consciousness of the upper atmosphere through its relation to radio waves, makes Dr. Stetson's book a most timely contribution to this subject.

In keeping the language simple and concise, the author has not always been able to treat his subject with absolute scientific accuracy and completeness, but these failings are expected and excused in such a popular and simplified book. For readers who are more scientifically critical, the book contains a most valuable bibliography and a complete table of monthly relative sunspot numbers since 1749.

SETH B. NICHOLSON
Mount Wilson Observatory
Pasadena, California

LECTURES

Current Trends in Psychology. Wayne Dennis *et al.* vi + 225 pp. $3.50. Univ. of Pittsburgh. 1947.

IN THIS little book are published eight lectures delivered at the University of Pittsburgh in March 1947. Seven specialties in the field of psychology are described, each by an expert engaged in the practice of the specialty he discusses. The eighth lecture, given by Dr. Dennis, is concerned with Psychology as a Profession and served as an introduction to the conference at which the lectures were presented.

The following specialties are discussed: Experimental Psychology (B. F. Skinner); Child Psychology (R. R. Sears); Clinical Psychology (E. L. Kelly); Psychotherapy (C. R. Rogers); Personnel Psychology (J. C. Flanagan); Human Engineering (C. T. Morgan); and the Sample Interview Survey (R. Likert). In most of the cases, the

writers define their fields and give brief historical backgrounds for them, but the emphasis throughout is on current trends and anticipated future developments. The style is readable and the treatment full of insight and thought-provoking.

To the reviewer, reading the book was a stimulating and pleasurable experience. He does not agree with everything that is said in these discussions, but disagreements here are largely matters of opinion. As a description of how far psychology has advanced in the past decade and as an indication of the activity in and the breadth and scope of the field, this book can be highly recommended. To profit most from it, the reader should have some background of knowledge about psychology. But for anyone it will serve to give at least a partial conception of, and basis of definition for, a field that is widely misidentified with everything from crystal gazing to a concern solely for mental testing.

CHARLES N. COFER
Department of Psychology
University of Maryland

A NATURALIST'S WIFE IN COLOMBIA

East of the Andes and West of Nowhere. Nancy Bell Bates. 225 pp. Illus. $3.50. Scribner. New York. 1947.

HERE is a charming and interesting account of life with "The Boss" while he pursued his scientific studies for the Rockefeller Foundation in faraway places. "The Boss" in this case is the naturalist, Marston Bates, the husband of Nancy Bell Bates, the author of this book. Mrs. Bates, daughter of the renowned plant explorer David Fairchild and granddaughter of Alexander Graham Bell, fits naturally into the sort of life she has chosen, both through native inclination and because of her childhood experiences in company with her father on some of his expeditions.

Mrs. Bates sketches in an easy and interesting style and with a pleasant sense of humor her experiences with "The Boss," first in Albania and Egypt while he studied the malaria problem, and then in Colombia during his research in the field of yellow fever. Stopping on their way across the Andes at "the cold tropics: Bogotá" for the arrival of their first child, the Bateses descend the eastern slopes of the mountains, over roads made more precarious by unpredictable landslides, to reach their station at Villavicencio on the western margin of the Llanos. In these all-but-trackless grass plains, reaching eastward and touched here and there by the jungle into which they are finally absorbed, is the "Nowhere" of the book's title.

The lives of the Bateses blend harmoniously with the native surroundings, the customs and traditions that Mrs. Bates sympathetically portrays in her intimate manner. The "goings-on" in the lab are represented as a delightful mixture of human relationships and objective scientific procedures. Animal pets of the strangest sorts and the children's play pen are as much a part of the laboratory as of the Bates home, and many are the interesting incidents that arise from such circumstances.

As may be surmised, the Bateses have a truly catholic interest in the country about Villavicencio. Explorations into primitive communities and the native fiesta fascinate them as much as the strange butterflies and monkeys; the plants of the jungle interest them along with the mosquito larvae they collected from the rain puddles along the dim, dirt roads of the tropical woodland.

A number of splendid photographs of the Villavicencio region suffer somewhat from the obvious attempt to economize in their reproduction. One could also wish that these illustrations had been distributed throughout the book adjacent to the subject matter instead of being lumped together in three batches. A detailed map of Colombia, especially of the vicinity of Villavicencio, would have added considerable interest, although the book easily holds the attention of those readers who enjoy vicarious participation in such experiences as are described.

DONALD EDWARD MCHENRY
Yosemite National Park
California

COMMENTS AND CRITICISMS

ON COMMENTS AND CRITICISMS

My father, who in his time contributed occasionally to the old *Popular Science Monthly*, said somewhere in his writings that if human beings were fully rational animals, they would all draw the same conclusions from the same given evidence. I am increasingly impressed that contributors to "Comments and Criticisms" and to the main body of THE SCIENTIFIC MONTHLY are either examining different evidence or are drawing different conclusions from the same evidence.

It might not be irrelevant to add an anecdote about my father: It must have been at the time of the Scopes trial, when a reporter recognized him as he was waiting for a Mount Pleasant car. Just as the car came along the reporter hailed him and said, "Dr. Spillman, do you believe men are descended from monkeys?" "Not yet," answered my father, and swung aboard the car.

RAMSAY SPILLMAN, M.D.

New York, New York

SCIENTISTS AND SOCIAL PROBLEMS

Are scientists who express opinions in relatively nonscientific fields—such as politics, sociology, religion, economics, or art—justified in ignoring the principles that they take for granted in scientific investigation? Or does the unique contribution that scientists can make to such problems lie in the scientific attitude—the objectivity, the willingness to consider all existing evidence and theory, the precise definition of meanings, and the scrupulous regard for logical consistency that we like to think are characteristic of the scientific approach? It should be superfluous to point out that scientists who merely use their professional eminence to gain a hearing for their unconsidered opinions and irrational prejudices do a service neither to science nor to society.

P. W. Bridgman had an article on "Scientists and Social Responsibility" in THE SCIENTIFIC MONTHLY for August 1947, and has set forth his views at greater length in a book called *The Intelligent Individual and Society*. His writings compare favorably with many similar articles being produced by scientists and engineers on such subjects: they are here singled out merely because Bridgman's standing as a scientist ensures their acceptance as representative of the best scientific thinking. One should certainly be justified in taking for granted that they will be well considered, logically consistent, and an advance over previous thinking on the subject. Is this the case?

"Scientists and Social Responsibility" begins by pointing out, irrefutably, that it is impractical to force a scientist to foresee the final results of his investigations, or to undertake the control of those results. It seems improbable, though, that any appreciable number of intelligent people ever assumed that this was possible, or had any such implications in mind when they spoke of social responsibility. To most of us, this implies simply an informed and active participation in the functioning of the society of which we are a part. It happens that the structure of our society is such that this does require the scientist to function to some extent as a politician, a sociologist, and an economist—but only to the extent that it requires this of any citizen who chooses to be influential in our society.

When Bridgman pictures the scientist as being bullied by society into taking up an intolerable burden, he appeals to our emotions rather than to our reason: the problem is not presented in that form. Power and responsibility go hand in hand, whether Bridgman likes it or not. The choice offered is simply whether the scientist, as he gains increasing control over the future of civilization, shall accept responsibility in proportion to his influence, or shall surrender both the responsibility and the power that goes with it to those who will be glad to use them for their own ends. The latter alternative will probably not be favorable to continued free inquiry and will certainly not improve the status of scientists as individuals.

So far as I can follow Bridgman's argument, he proposes to solve the problem by changing society to fit the scientist. This can hardly be considered a simple expedient: it certainly implies far more active involvement of the scientist in social dynamics than the solution he rejects. In any case, it seems to imply that the ideal society would be one in which no one had any responsibility except for his own specialty: certainly the argument that Bridgman advances for scientific irresponsibility could equally well be advanced by every other group in our society. His principle of "the general ideals of the specialization and division of labor, and the ideal of, as far as possible, each man to his best" leads logically to a state in which all politics, all economics, all religion, all philosophy, are controlled by self-appointed specialists in each field—since no group has any responsibility for any field but its own, and therefore no power in any field but its own. This is the concept that the human race has been fighting ever since the Middle Ages—it would wipe out the results of the American Revolution, the French Revolution, and every other struggle against the theories of divine right, hereditary aristocracy, and theocracy.

The argument is confused by the fact that Bridgman rejects any "metaphysical concept of society as some superthing, transcending the individuals who compose it. . . . Society is composed of you and me;

society does not have an individuality of its own, but is the aggregate of what concerns you and me." He then goes on to argue as though society had just such an individuality of its own: "Society can deal with the issues raised by scientific discoveries by other methods. . . . If it had not wanted to construct an atomic bomb, it need not have signed the check . . . which alone made it possible. . . . Let society deal with this situation by the means already in its hands. . . ." Who deals? Who signed the check? If society is composed of you and me, and you and I are irresponsible, who acts for society? Specialists like Hitler?

His concept of society as "composed of you and me" and nothing more seems more open to criticism, though, than any inconsistencies in his development of the argument. If anything can be said to be self-evident in the field of social theory, it would seem to be the fact that society is very much more than just "you and me"—that the whole is very much more than the mere sum of its parts, and that it differs significantly and qualitatively from such a sum.

When Bridgman talks of "you and me," it is obvious that he is not thinking of Bridgman the animal but of Bridgman the Professor, with all the education and background that he has received from society. But Harvard professorships and Nobel prizes and physical science were no part of Bridgman's germ plasm: they are social inventions, conceivable only in a large and complex and wealthy society. A society that really consisted of Bridgman and me, with none of the social traditions and values that he takes for granted, would be a repulsive thing. We should certainly not have a language or a number system. We should almost certainly not have fire or clothing or soap or weapons. We should spend damp, cold, miserable lives grubbing for enough roots and maggots to keep us alive, in a world without philosophy, religion, art, or science. If a female were in the neighborhood, we should probably spend our spare time throwing rocks at each other to impress her—or, more probably, Bridgman would be smart enough to think of throwing rocks, but I, less talented, should only bite him.

How many such creatures would it take to make a civilization?

Bridgman's refusal to see society as anything but a fortuitous juxtaposition of predators, most of whom have discreditable intentions toward Bridgman, probably accounts for his strange obsession with the idea that he is being exploited—that he must make a last-ditch stand against "the right of stupid people to exploit the bright ones." Just how such exploitation is possible, he does not adequately explain. Certainly, if he wishes to avoid contributing more than a minimum to society, he can always outwit us by taking a job as a janitor or day laborer—there is no way that a creative individual can be forced to create against his will. Strangely, though, what he seems to be opposing is any limitations on his activities as a scientist. If his scientific contributions are evidence of exploitation, this puts him in the strange position of defying his exploiters to deny him the privilege of being exploited.

Bright people have always been exploited by stupid ones, if exploitation is defined as contributing more than an average amount of creative work to a culture. It is improbable that even the greatest man has ever given society more than he received from it, however: the best of men can only make the world a very little better, but the worst society offers the individual the difference between being an ape and a human being. In any case, the greatest geniuses do not seem to have been embittered by the feeling that they were being exploited, nor felt their genius to be an unsurmountable barrier to leading a satisfying life. On the contrary, most of them seem to have found a very deep source of satisfaction in the very superabundance of their creativity. Is the individual exploited when society gives an individual the opportunity for the activity that is necessary to the complete expression of his personality? Does he profit when it refuses to accept his potential contribution?

The concept of life as a sort of perpetual feud between the individual and the human race seems to underlie much of the analysis in *The Intelligent Individual and Society* and to account for such conclusions as that on page 230: "This discussion must have made it particularly plain that the interests of society and the individual do not coincide by any means, although such has been obvious all along whenever we have scratched beneath the surface." Or that on page 281: ". . . I have got to recognize that in the conflict between the individual and society the only thing that makes sense for the individual is to get away with what fulfills his purpose. . . ." Bridgman is not very convincing, however, when it comes to pointing out just what antisocial "interests" the intelligent individual should promote, or how "getting away with" antisocial acts is the best way to ensure lasting gratification of the sort of tastes that an intelligent individual should develop. He seems baffled by the problem of the intelligent individual who finds that he gets more satisfaction out of living by burglary than out of any law-abiding career open to him. ("Would not the man be a fool if he did not take to burglary?" asks Bridgman.)

Strangely, it never seems to occur to Bridgman that his individual might, desirably, be educated as well as intelligent. If he were, he certainly would be aware of the fact that there may be serious consequences—for the individual as well as for society—in spending one's life in the consciousness that one is waging a sort of undeclared war on one's fellowmen. He might even have read Dostoevski—at least *Crime and Punishment*. He could very well be conscious of the fact that the great drawback to a life of crime is not possible retaliation by society, but the loss of almost every value that intelligent individuals have found worth living for. He would not

be convinced that the juvenile thrills of playing cops and robbers were so lasting a source of satisfaction as to justify giving up, for their sake, all hope of honor or prestige, all sense of security or of achievement, all prospect of association with people whom he could respect or trust.

Bridgman's unawareness of alternatives to his proposals is partly explained by the Preface, in which he states that his reading of philosophical literature has been very limited. One can only wonder how he would react if one of his graduate students prefaced a thesis with the remark that it ignored all developments in the field of physics since Aristotle.

If Bridgman's nonscientific writings fall short of the scientific ideal, Du Noüy's are a headlong attack on that ideal. His recent book, *Human Destiny*, received a very sympathetic review in THE SCIENTIFIC MONTHLY for May 1947. It has had an enormous sale and almost certainly is now accepted by great numbers of Americans as an authoritative presentation of the latest scientific attitudes toward religion. As far as I am concerned, it is so bad as almost to defy serious criticism. It purports to be a scientific proof of the existence of God, but it is neither scientific nor a proof: the premises from which Du Noüy argues are questionable, his arguments illogical, and his conclusions largely meaningless.

As an example, Du Noüy sets out to prove that life could not have originated by chance. He takes as a model 1,000 black particles shaken with 1,000 white particles, and uses this model as the basis for considering the probability that a molecule with 2,000 constituent atoms and a degree of dissymmetry equal to 0.9 could originate by chance. He decides that the chance of such an occurrence is only one in 2.02×10^{321} and dismisses this as "impossible in the human sense." Now, molecules do not originate from atoms clumping together at random like damp cough drops: if this were the case, there could be no science of chemistry. Even if they did, it does not mean that one chance in 2.02×10^{321} is the same thing as impossibility: if it were, a poker game would become impossible after about fifty hands, since the probability that one will hold the precise sequence of five-card hands which one does hold is only one in $2{,}598{,}960^n$ where n is the number of hands. Finally, the argument would prove nothing of value even if it were valid: if God exists, the laws of probability are presumably as much a manifestation of his nature as is the existence of life. God is no greater and no less, no more understandable and no more inscrutable, if one assumes that he chose to originate life in one way or in another.

Similarly, Du Noüy's "proof" of the teleological basis of evolution amounts to little but the statement that there must be a purpose in nature intelligible to Du Noüy—because otherwise nature is incomprehensible to Du Noüy. This used to be called tautology.

Worst of all, Du Noüy has made a substantial contribution to misunderstanding by implying that he is defending God against a determined attack by "materialistic scientists" who are actively engaged in destroying "in the name of a science still in the cradle, the doctrines which up till now gave a meaning to individual life . . . namely, the religions." I know of no scientist who will not admit that there are relationships—and probably forces, particles, and unimagined sources of phenomena—of which scientists still know little or nothing. If Du Noüy wished to define God as that which was implied by this ignorance, he was at perfect liberty to do so. His argument seems to lead to a concept that is essentially the same thing—and one that is just as useless. But why must God be identified with ignorance, and understanding be regarded as atheism? Why does the believing mind find it intolerable to imagine that God should choose to work through what we call natural laws, instead of being utterly arbitrary and unpredictable? What denial of divinity is implied by the discovery that the fragment of time and space we have explored seems, to some extent, orderly and predictable?

If Du Noüy's reasoning is, as he claims, scientific, science might well be the menace he implies. Fortunately, this is not the case. Unfortunately, he has convinced many that it is.

WM. PALMER TAYLOR

Hamilton, Ohio

THE MIMA MOUNDS

Victor B. Scheffer's article, "The Mystery of the Mima Mounds," which appeared in the October 1947 issue of THE SCIENTIFIC MONTHLY, raised some questions that went unanswered.

Since I had considerable experience with the pocket gopher in east Texas and in Louisiana, as well as in a number of other states, I shall attempt to answer some of the more important questions raised that went unanswered, and interpret differently one or two other factors the author attempted to explain.

It is customary for the pocket gopher to resort to mound building in areas where the soil is inadequately drained, where water is likely to stand on the surface of the ground for considerable periods of time under favorable conditions. The purpose of the mound in this case is to obtain adequate drainage for the nesting chamber. That also is why the rodent consistently pulls the dirt toward the nesting chamber, which is just beneath the surface at the highest elevation on the mound. This habit of continuously pulling the dirt toward the nesting chamber, incidentally, accounts for the mound itself, as generation after generation of the rodents continue to inhabit the same area.

Apparently, however, the motivating factor for mound building by the pocket gopher in the Puget Sound area is somewhat different. Here, according to Dr. Scheffer, the soil originally was composed of a thin stratum, or layer, of silt over a gravelly subsoil. The water storage capacity of this thin layer

of soil was not sufficient to sustain vegetative growth during prolonged dry spells. Since the pocket gopher feeds on roots and other underground portions of growing plants, he was confronted with the choice of abandoning the area or of correcting the situation. He elected to do the latter. By increasing the depth of this layer of silt, at least in patches, by working it into a mound, always working the dirt toward the nesting chamber at the crown of the mound, the gopher could build up the water storage capacity enough to meet this requirement. By working the stones in the lower edge of the silt layer to the bottom and ejecting those near the surface from the soil mass, he could improve the water storage reservoir still more.

Contrary to Dr. Scheffer's expressed opinion that the pocket gopher dug deep into the gravel for safe nesting chambers, it is my opinion it was for the purpose of increasing the depth of the soil mass to ensure sufficient water storage capacity in the soil for plant growth. As previously stated, the nesting chambers appear immediately beneath the surface on the highest portion of the mound.

The rodent depends on the maze of tunnels leading to and from the nesting chambers for escape when in danger of being attacked by enemies. Several of these tunnels lead out in as many directions from the nesting chamber.

Insofar as my experience goes, the pocket gopher does not resort to excessive mound building in areas where the soil is deep and well drained.

J. H. STALLINGS
Soil Conservation Service, U.S.D.A.
Washington, D. C.

OMISSION

Dr. L. J. Meduna and Dr. J. R. Klein, Illinois Neuropsychiatric Institute, University of Illinois, Chicago, are the clinical and chemical investigators, respectively, on glucose metabolism in acute psychoses, isolation of an anti-insulin factor in schizophrenia, and the use of carbon-dioxide inhalations in the treatment of several forms of mental disorder. See "Science in Chicago," December 1947 issue, page 479.—Ed.

SHAKESPEARE'S PHYSIOLOGY

Noting Dr. Bradford Willard's article (November SM), I was struck by his reference to the omniscience of Shakespeare.

I felt much the same on several occasions while reading certain passages of William.

Dr. Willard refers to geological matters, but I have wondered how many scientists and medical men have realized that Shakespeare showed rather an uncanny ability, and had more than a little knowledge of many subjects that had no connection with the usual author's writing, especially in his time.

I draw attention to his passage in *Coriolanus* that shows that he must have been a great reader, or that he must have known personally many who delved into questions not usually of interest to authors. Harvey had only recently (or perhaps later), given his understanding of arteries and the circulation of the blood, whereas prior to Harvey it was believed that arteries were filled with air.

Here is what the layman, Shakespeare, said (italics mine):

"True is it" [quoth the belly]
"That I receive the general food at first,
Which you do live upon; and fit it is,
Because I am the storehouse and the shop
Of the whole body: but, if you do remember,
I send it through the *rivers* of your *blood,*
Even to the court, the heart, to the seat o' the brain;
And, through the *cranks* and offices of man,
The strongest *nerves* and small *inferior veins*
From me receive that natural competency
Whereby they live."

The foregoing shows an extraordinary interpretation of what was not very well known, even to many students of science, at that time. Shakespeare must have been an omnivorous reader, or associated with many men of letters, as of science.

FRANK E. DORCHESTER, M.D.
Vancouver, B. C.
Canada

TECHNOLOGICAL NOTES

Moisture barrier. Dry heat and dry cold are always more comfortable than the same temperatures coupled with humidity. The reason heat feels better dry than damp is evident, but the why of discomfort from damp cold is not so well understood. March is a good time to recall the reason, given in an American Chemical Society paper last year. Moisture vapor diffuses through clothing faster than the molecules of nitrogen and oxygen do and carries body heat away rapidly. The suggested remedy when the air is cold and damp is a raincoat over the woolen garments, even if it is not raining.

City lime. Discovery that city water is softened with lime surprises some citizens who have heard that lime makes water hard. Some fear that every drink will load their bodies with chemicals. The chemistry is easily explained; the addition turns the hardness into insoluble calcium carbonate, or limestone, which settles out. City waterworks, although they produce limestone in the settling basins, are steady customers of the lime companies. New methods of drying the limestone sludge for agricultural purposes, and firing it to make lime that can be used in the softening process, may change the city waterworks from customers to competitors in the lime business.

Protest. Curiosity and a lively interest in new developments are certainly characteristic of most SM readers. We may—and we should—marvel at neat ways to turn nature's laws to advantage. But explanation rather than expression of amazement is proper for a discussion of such manipulations.

What prompts this observation is a release containing an irritating use of the word "wonder." This time it's about the mass spectrometer, "wonder tool of science," for detecting tiny leaks in gas-filled plumbing. A booklet about soybeans bears the title *The Wonder Bean.* A few years ago articles on sulfanilamide or penicillin often referred to them as "wonder drugs." Let's have done with this wonder business.

Gas detector. In garages, mines, and other places of employment there is frequent tragedy from carbon-monoxide poisoning. Two ingenious devices, recently reported, are said to give warning of dangerous concentrations of the gas.

One detector draws the air sample over a catalyst that oxidizes all CO to CO_2. When a predetermined setting is reached in the heat produced by this chemical reaction, a horn blows and a red light goes on.

The other device is described as "an electronic ear." A transducer sends sound waves through a tube of gas. Presence of CO—or other dangerous gas—changes the speed of the waves. When the difference is enough to denote dangerous concentration, the alarm sounds.

Buried danger. Fought-over land in Europe has to be freed of unexploded shells before it can be farmed with safety. Similar dangers exist in timber that has been hit in artillery range work; unexploded ammunition is dangerous, and buried metal plays hob with saws. An electronic coil, five feet in diameter, is used for testing suspected timber. The device indicates electrical unbalance when a log containing a piece of metal floats through it. More than 12 million board feet from 3,700 acres in the Ft. Lewis Military Reservation, Washington, will be salvaged by this method.

Country lights and phones. Electricity has come into use in rural districts more than the telephone has. Census figures give 1,385,000 farms with current but no phone, and only 464,000 farms with phones but no electricity.

Useful trolley. Disappearance of the street car doesn't help traffic congestion. Automobiles move fewer than two persons each in city traffic. Three lanes of automobiles will move only 2,700 persons per hour, but replacement of one lane of automobiles by street cars increases the number moved to 15,630 persons per hour.

M. W.

THE BROWNSTONE TOWER

MORE than a year has passed since we moved from our office in the Brownstone Tower of the Smithsonian to the A.A.A.S. Building at Scott Circle. We hardly need the enlarged picture of the Tower, which hangs in our present office, to remind us of the unique features of our room behind the clock face. Last fall I visited our old office, which is now occupied by Dr. Abbot's computer and is appreciated by her as much as it was by us. I had wanted also to see our host of the Tower, Dr. Earl S. Johnston, whose office was two floors above ours, but I had to hurry away. Now I know that I should have taken time to find him, for that was my last opportunity to see him. Dr. Johnston died on December 17, 1947.

I have called Dr. Johnston our host of the Tower because most of the rooms in it were a part of the quarters of his Division of Radiation and Organisms. He was a generous host and added much to our pleasant memories of the Tower. With my back to the elevator I could tell when he was going by, for my assistant would greet him with a smile and a wave of her hand. Now and then he would stop at our floor and come in for a chat. So often he had a humorous anecdote to tell that we would ask him for a yarn or a joke and were rarely disappointed.

The research of Dr. Johnston and his staff in plant physiology was carried on in the rooms of the west basement of the Smithsonian Building. One day he personally conducted us through his laboratories, explaining what was being done in each room and the intricate equipment that had been devised for the work. We registered the awe and admiration that any layman shows toward apparatus and methods that he does not fully understand. When the time came to prepare for our Smithsonian Centennial Issue (November 1946), we asked Dr. Johnston to write an article on the Division of Radiation and Organisms. When he promised to do it, we did not have to worry about its prompt delivery, for he was a man of his word. The nature of his work in the low-ceilinged basement is well described in that article.

The son of a Methodist minister, Dr. Johnston was born February 5, 1889, in Quarryville, Lancaster County, Pennsylvania. He graduated from Dickinson College, which was also the alma mater of the second secretary of the Smithsonian, Spencer F. Baird. In later years he would return on vacation to the beautiful countryside of southeastern Pennsylvania. It was obvious to all that he found life "full of a number of things." I have recently learned that his principal hobby was photography, and the money that most men, including the editor, waste on tobacco, provided film for his camera. He was a collector, too—of stamps, coins, etchings, and well-chosen books. Being a devotee of chess, he also collected ornamental chessmen.

The cause of Dr. Johnston's death was cancer of the lower alimentary tract. We did not know the nature of his illness at the time of his first operation. When he returned to the office, he told us cheerfully that his inner tube had been vulcanized. A second operation was required last year, and later we learned that he could never return to the Tower. His bright courage persisted to the end and he made plans and gave advice for the work of his division after he knew that he could not recover. His last view of the beloved Smithsonian came to him in his home when a motion picture, Treasure House, delineating the present activities of the Smithsonian, was shown for him. Thus a blithe spirit departed from the Brownstone Tower.

F. L. CAMPBELL